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DIRECTIONS OF PROGRESS IN ECONOMIC GEOLOGY[1]

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THE subject chosen for my address this morning is of rather general character and one that should permit a retiring president to exercise his privilege of indulging in retrospective as well as in prospective views and of expressing ideas which, although possessing little novelty, may, nevertheless, be worthy of consideration. My own work in economic geology has been mainly with ore deposits, and if in the present address that particular aspect of geology seems to receive disproportionate attention, this is due to the limitations of my own experience and not to any desire to minimize the significance of geology in relation to the occurrence of oil, coal and other non-metallic resources and to problems of engineering in connection with the construction of dams and reservoirs, the driving of tunnels, and other projects in which the character and structure of the rocks and the geologic history of the region may be of fundamental importance.

As we look back over the past fifteen or twenty years, it does not appear that we have made such progress as will mark this period as particularly noteworthy in the history of our science. The recognition of the predominant part played by thermal water in ore deposition; the outlining of the processes of metasomatism; the perception of the close connection between ore genesis and igneous intrusion; the discernment of the general zonal arrange-

[1] Presidential address delivered before the Society of Economic Geologists, Cleveland meeting, December 30, 1927.

ment of ores in relation to igneous intrusive masses; the discovery that the minerals of sulphide ore bodies have usually formed in a more or less orderly sequence; and finally, the arrival at a fairly satisfactory state of knowledge concerning supergene enrichment, especially of copper and silver ores—these all date from an earlier period than that to which I have just referred.

The general tendency during late years has undoubtedly been to regard ore deposits as more and more closely linked in origin with the process of magnetic differentiation in igneous rocks, and there are brilliant enthusiasts among us who decline to see any difference between a vein and a dike. Perhaps there is no absolute division between the two, for, after all, classification is a human device and we are frequently impelled to draw lines where nature has left more or less shadowy zones of gradation. Nevertheless, at the risk of being considered old-fashioned, I wish to express adherence to the view that most ore deposits are the work of mobile aqueous solutions, basing this conclusion particularly upon experience with the gold ores of Cripple Creek; the lead-silver replacement deposits of the Cœur d'Alene district, in Idaho; the veins of Butte; the great displacement copper deposits of Bisbee and Jerome, Arizona; the disseminated copper deposits of the southwestern United States; the gold deposits of California, of Oatman, Arizona, and of Goldfield, Nevada; the quicksilver deposits in various parts of the west. To one who holds such a view, the introduction of the terms " veindike " and the equally uncouth " magmation " is naturally not to be considered as a milestone in the progress of economic geology.

Precisely as was the case with older views, the modern protagonists of the magmatic origin of ores have sought to reduce the formation of all ore deposits to the operation of one major process. They hold that where sulphide ore is present, there must be present also, whether visible or not, the intrusive rock that is representative of the magma whence the ores came. Possibly they are right but their evidence is unconvincing. Not to speak of many deposits of lead and zinc sulphides and some copper ores that appear to lack any obvious genetic relationship to intrusive

rocks, some of the Tertiary gold-silver veins in lava flows are not clearly dependent upon igneous intrusions. They possess rather characteristically the disconcerting habit of disappearing at moderate depths with little to indicate downward continuity with a deep magmatic source. Examples that come to mind are the veins of Oatman, Arizona and of Mogollon, New Mexico. Even where igneous rocks are visible we frequently find it difficult to agree as to which rock has a genetic relationship with the ore. In the Jerome district, Arizona, for instance, some geologists have considered the deposition of the pre-Cambrian copper ores to be related to the intrusion of the Bradshaw granite, the nearest exposure of which occurs several miles away; others have concluded that the so-called Cleopatra porphyry represents the magma from which the ores came. My own recent study of the district has led me in turn to the conclusion that the Cleopatra porphyry is in the main an ancient rhyolitic flow and that the intrusion of the augitic Verde diorite was probably the event most closely connected with ore deposition.

It is not implied in the statements just made that the hypothesis or theory that most sulphide ores are of direct magmatic origin is incorrect, but my thought is rather to suggest that something yet remains to be done to establish it on a sound basis as a generally inclusive explanation.

Among the distinct advances of recent years should be mentioned the accumulation of evidence of the zonal distribution of ores of particular metals around intrusive igneous masses. The idea itself is probably at least 75 years old but was revived during the early part of the present century by De Launay, in connection with the conception of metallogenetic provinces, and has been found applicable, in a broad way, by later investigators to the deposits of many regions, in all parts of the world. Recent papers by W. H. Emmons,[2] setting forth evidence for the zonal grouping of ore deposits and proposing a classification of deposits, based upon position with reference to the related intrusive mass, are particularly noteworthy.

[2] Emmons, W. H., "Relations of Metalliferous Lode Systems to Igneous Intrusions." *Trans. A. I. M. M. E.*, vol. 74, pp. 29–70, 1927.

The work of Augustus Locke and his assistants, on the interpretation of gossans, also deserves notice as a courageous attempt to solve a difficult problem of great practical importance. There is a considerable element of empiricism in their work and the interpretation of oxidized outcrops in terms of original sulphides appears at present to demand special and long experience that few geologists have acquired. Nevertheless some success has been achieved and there is ground for hoping that further progress may be made in this hitherto neglected field. Locke, Blanchard, Boswell, Kingsbury, and others who have collaborated in this work, deserve the credit of being pioneers in a promising line of investigation.

Certain advances in the technique or methods of economic geology should also be referred to in any general summary of progress. Various electrical methods of prospecting have in recent years gained recognition as being useful under certain conditions and have triumphantly emerged from a status of supposed close relationship with that curious evidence of human credulity, the forked rod of hazel. This application of physics to practical geology is still young but has demonstrated its capacity not only to detect the presence of bodies of metallic sulphides but also to work out geological structure and thus to become an aid in the exploration for oil and other non-metallic substances. Similar comment might be made with reference to the torsion balance and seismograph, now used in prospecting for oil, especially where it occurs in the vicinity of salt domes.

In the same category of advancement in technique should be noted also the development and application of what has been called micro-paleontology to the identification of strata penetrated by the drill, whereby use is made of foraminifera and other minute organisms that can be recognized in the drill-sludge.

Having briefly referred to some of the recent advances in economic geology, I come now to the question—Why has progress during recent years not been more rapid?

The principal reason, as I see it, is the lack of fundamental chemical and physical data. We have been accumulating a vast

store of observations on the results of geologic processes and have been indulging in more or less intelligent guessing as to the nature of the processes themselves, many of which have operated under conditions that must have been far removed from anything within the range of our own experience. We argue on the one side, for example, that the silica of certain quartz veins was deposited from mobile fluid solutions, and on the other side that it must have shouldered its way into its present position as a stiff or gelatinous magma. We may perhaps advocate a compromise view, namely, that although originally deposited from thin solution, the silica passed through a gelatinous or stiff condition, with possibly various movements and stages of crystallization before its final solidification as quartz. We may fancy that we have evidence for one or other of these views, but I believe that our opinions, except perhaps for veins of one or two characteristic types, are largely conjectural. As a matter of fact, we do not really know how silica, water, and metallic sulphides, or their constituents, behave at the temperatures and under the pressure that must have prevailed during the formation of many ore-bodies.

That the behavior of silica has great practical importance is illustrated by a problem that is now pressing for solution in the Bisbee district, Arizona. Most of the known ore bodies in that district appear to have a fairly close connection with masses of quartz that have replaced the Paleozoic limestones irregularly and outcrop at the present surface. Similar, if not identical, masses of quartz occur in outlying parts of the district, some miles from any known ore. Is there ore within practical mining distance beneath them? Do they represent the devious migration of siliceous solutions beyond the region of ore deposition? Have they any connection whatever with ore deposition? The future of a great copper district depends upon the answers to these questions. The geologist may have an opinion about this problem but he cannot answer the questions with the sureness and precision of real knowledge. In the meantime, extensive underground exploration is in progress in an attempt to settle the problem in a practical but expensive way.

To take another example, we have discussed for many years the question of the origin of the nickel-copper deposits of Sudbury and are not yet agreed whether they have merely settled from the associated norite magma by a more or less hypothetical process of differentiation, have been deposited by hot or thermal water, or by some combination of the two processes.

Again, leaving for the moment the subject of ore deposits, we, as economic geologists or as geological engineers, are sometimes asked to determine whether a certain reservoir site will hold water behind a dam 300 or 400 feet high. As far as I know, there are no experimental or quantitative data on record that will furnish a basis for answering this question. The geologist must make the best prediction that he can under the circumstances, perhaps aided by some knowledge of the behavior of reservoirs previously filled in similar rocks.

Are we to conclude that it is impracticable to obtain more definite information to help us to answer these and other questions in economic geology, or must we continue to observe and record the end results of geological processes and remain content to guess at the nature of the processes themselves? I firmly believe that if real progress is to be made in the theory of ore deposition we must turn our attention more and more to the task of obtaining, in collaboration with chemists and physicists, a larger body of fundamental information than we now possess.

The recent papers by Boydell,[3] in which he discusses critically the probable action of colloidal solutions in ore deposition and the physical chemistry of metasomatism, are steps in the right direction, but we require more experimental work on these same problems.

Experimental work of the kind needed is beyond the resources of most individual investigators and demands also such continuity of effort as cannot be confidently expected of a single worker, for whom favorable working conditions may so easily be de-

[3] Boydell, H. C., "The Rôle of Colloidal Solutions in the Formation of Mineral Deposits," *Inst. Min. and Met.* (London), Dec., 1924. "Discussion of Metasomatism," ECON. GEOL., vol. 21, pp. 1–55, 1926. "Operative Causes in Ore Deposition," *Inst. Min. and Met.* (London), Oct. 27, 1927 (advance copy).

stroyed by an unexpected turn of events. It might, however, be undertaken by an organization created and endowed for the purpose or by such an existing organization as the Geophysical Laboratory of the Carnegie Institution of Washington, which is not under political control and which consequently is not hampered by any necessity to produce real or apparent results before the next appropriation. To those who might object that such work would be a falling away from the ideals of pure science, it is only necessary to point out that scientific work loses nothing in spirit or method merely because the results sought are known to have direct and immediate utility. The old notion that a scientific investigation undertaken with prior knowledge that the results desired will be useful or beneficial must in some way be inferior to one in which the application of the results is as yet unknown, is a fallacy which was thoroughly discredited by the achievements of Pasteur.

Among the particular problems upon which light might be thrown by research from the physical and chemical side a few only will here be briefly mentioned. One of the first that comes to mind is that concerning the nature of the solutions that carry and deposit sulphide ores, if it be admitted that such ores are deposited from solution. From some old experimental work by Becker and Melville, from our knowledge of hot-spring waters, and from the metasomatic changes effected in the wall rock of veins, we generally accept the view that most veins carrying metallic sulphides have been deposited from alkali sulphide solutions. For the lead-zinc ores of the Ozark region, Siebenthal [4] has argued for "alkaline-saline sulphureted waters," the metals being carried as bicarbonates and thrown down under conditions that permitted the escape of carbon dioxide. We need more definite information concerning the capacity of various possible natural solutions to carry ore constituents and concerning the chemistry of ore deposition. Some of this information at least could be supplied by experimental work.

[4] Siebenthal, C. E., "Origin of the Zinc and Lead Deposits of the Joplin Region," U. S. Geol. Survey Bull. 606, p. 161, 1915.

Another problem for the geophysicist and geochemist, as Lindgren [5] has pointed out, is to explain the observed general succession in the deposition of the minerals in complex ores, the younger minerals not only following the older in more or less orderly sequence but also successively replacing them.

The question as to the behavior of silica in ore deposition has previously been mentioned. Our difficulties with silica, however, do not cease with the original deposition of the ores. In many gossans, such as occur above the great copper deposits at Bisbee and Jerome, Arizona, there appears to be more quartz than was present in the original ore. This raises the questions: To what extent is silica dissolved and redeposited during the oxidation of sulphide ore bodies, and what is the chemistry of the process? The answers to these questions have an important bearing upon the interpretation of the oxidized outcrops of ore deposits.

Certain lead deposits, particularly the great *manta* deposit at Los Lamentos, Mexico, show a remarkable concentration of wulfenite and vanadinite in the lower part of the oxidized zone. Is this a downward concentration of small quantities of molybdenum and vanadium originally present in galena and, if so, why are these constituents concentrated near the level of underground water?

If most sulphide ore bodies are magmatic differentiation products from intrusive igneous rocks, it would appear that quickly chilled, glassy volcanic rocks should contain a larger proportion of the metals than do their plutonic equivalents. If this be true, have we not a suggestion for the origin of some deposits in Tertiary volcanic flows other than that of direct ascent from some hypothetical magmatic reservoir? So far as I am aware, no thorough investigation has been made to determine this point. Of course the quantities or proportions of metal involved would be extremely small and the work would require special analytical methods and a very large number of determinations.

Another question relates to the enrichment of sulphide ore deposits, particularly of copper deposits. Does the process take

[5] Lindgren, W., "Magmas, Dikes and Veins," *Trans. A. I. M. M. E.*, vol. 74, p. 89, 1927.

place only below the surface of underground water, or is it possible that in arid regions, where the supply of water from the surface is scanty, the surface of underground water lies deep, and sulphides are abundant, oxidation may cease some distance above the standing level of underground water and enrichment take place. The wide range and often irregular character of some of the enriched deposits in arid regions appear to hint at such a possibility.

The whole matter of metasomatic replacement, a process of which the results are obvious enough from the geological side, needs investigation from the chemical and physical side.

I might go on and cite other examples of the kinds of problems in which the cooperation of chemists and physicists is necessary if we are to solve them but such citation would probably be tiresome to many in my audience. Enough have been mentioned to indicate the general directions along which further progress in economic geology appears to lie. I shall now turn to another phase of my conveniently elastic subject.

In any consideration of the progress of economic geology it is appropriate on such an occasion as the present to inquire how our own Society can be effective for good.

I believe that one of the most important functions of the Society of Economic Geologists is to keep us in close touch with the scientific side of our profession, rather than with the industrial or merely economic side. In more or less intimate affiliation as many of us are with the development and exploitation of mineral resources, there is real danger that we drift away from the obligation to contribute to that branch of science which we are applying to utilitarian ends. It is from this point of view that comes the advantage, as it appears to me, of meeting, as we are now doing, with the geologists rather than with those who are interested primarily in the engineering and financial aspects of the mineral industries. Unless we remember that we are primarily scientific men we are likely to become content with the accomplishment of the particular job in hand and to lose sight of, or become apathetic towards, the broader consideration of our

relation to scientific progress. We may possibly become mere users of the results obtained by others, without making any contributions of our own. We are drawing upon the scientific capital of geology and adding nothing to the principal. It should be a function of our Society to counteract this tendency and this it may do in various ways.

In the first place, it can demand such qualifications for membership that only those may be admitted to its ranks who have shown both the ability and the determination to contribute worthily to the literature of geology. Men who are not primarily geological investigators, who are concerned merely with the routine or industrial side of their profession, should not be regarded as prospective members, no matter how successful they may be in the usual sense of the word. In its choice of officers and representatives, the Society can make plain to all the ideals by which it sets most store.

In the second place, it can show by its prevalent affiliations with other organizations, by the character of its meetings, by the nature of its discussions, and by the tenor of its publications that it is essentially a scientific society and not a technical or industrial institution. This attitude can not fairly be ascribed to any unworthy spirit of exclusiveness but should properly be interpreted as loyal adherence to a particular purpose and ideal.

The maintenance of such an ideal and the diffusion of its spirit among our members should go far to bring about a more tolerant attitude on the part of mining companies and other business organizations toward the publication of the results of geological investigations conducted primarily for their own purposes. It must be admitted that occasionally a mining company, especially under the often unpredictable consequences of our unfortunate mining law, may have occasion to regret some particular publication; but it is believed that, on the whole, the publication of scientific results is more likely to be beneficial than injurious, even to individual mining companies. I am glad to say that at least some of the companies with which I have had some connection hold this broad view.

Another advance towards which we as a society can probably contribute is that of coöperative geological work in mining districts. I am familiar with at least two districts in which the leading mining company maintains a strong geological staff whose members are likely to know more about the geology of neighboring mines than do those responsible for their operation. In others districts there are two or more independent geological staffs, each duplicating to some extent the work of the other and each in possession of some information that the other lacks. There may be no particular effort at secrecy yet there exists, nevertheless, a lack of complete mutual understanding. For example, in two adjoining mines that have recently occupied my attention, a particular group of rocks that is known in one mine by a certain name is entirely different in character and origin from the group known by the same name in the other mine. Furthermore, the geologists working in the two mines did not appear to be aware of this difference. Incidentally, the name is a binomial and recalled to me the remark ascribed to some historian with reference to the Holy Roman Empire, to the effect that it was neither "holy," nor "Roman," nor an "empire." It is to be hoped that the time will come when the companies in a mining district will combine to maintain an adequate geological department, whose members shall have access to all workings and whose results shall be available to all. There are of course obvious difficulties in the way of such a plan but they do not appear to be insurmountable. One step towards such an arrangement will be the recognition by mining companies that geologists as a class are men of high ideals with respect to truth, impartiality and duty. It is towards the attainment of such a standard that our Society can direct its influence.

It is natural that mining companies, who call in a geologist to advise them, should wish to have results as quickly as possible. The consequence is that, in many instances, far too little time is permitted for a thorough study. The visiting geologist is sometimes expected to accept in considerable part the facts as laid before him and to announce his decision. Probably in no branch

of science is first-hand knowledge so important as in geology and, although the geological maps and structure, as worked out by the company's regular geologists or engineers, may prove to be highly useful in directing attention at once to particular features, they cannot safely be accepted as a basis for important decisions. It is not uncommon to find such work vitiated by some preconception, or defective in that it has failed to take account of certain facts that are displayed somewhere else in the region although perhaps not in the particular area or mine that is under consideration. Too close attention to some small area of ground with neglect of the general geology of the district or region as a whole, is a very common mistake on the part of mining geologists, due partly to defects in their own training and partly to inability on the part of their employers to recognize the desirability of a wider outlook.

As members of the Society of Economic Geologists, supported by the conviction that the Society as a whole stands for scientific research, we can all do our share towards improving the conditions just outlined, especially by convincing our clients of the desirability of granting sufficient time for a thorough study of the problems set before us. Both by communication and by example we can be influential in effecting improvement in the circumstances and character of economic geological work.

Finally, may I add a few comments on the subject of teaching economic geology; for, obviously, much of the progress of the future must come from those who are now passing through our universities.

If it is true that further progress must depend largely upon the fuller understanding and use of physical and chemical data, it is more than ever important that the coming geologist should be well equipped in physics and chemistry, which presupposes, of course, mathematics. He should be led, furthermore, to realize that no one can truly become an economic geologist without first becoming a good geologist—a fact that has sometimes been overlooked. The economic geologist, provided that his work is not limited to the purely scientific study of mineral deposits, may

properly be regarded as a geologist who has accepted an additional responsibility, namely, that his conclusions be so sound that decisions involving large capital outlay may be based upon them. He has a double responsibility—first to science, and, second, to his clients. Although of course this should not be true, I cannot help feeling that sometimes "pure" geologists would be more careful in announcing their conclusions if they knew that these were to be tested forthwith by the expenditure of some thousands of dollars or that the success or failure of a great industrial enterprise depended upon their interpretation of some particular structure or the truth of their hypothesis of ore genesis.

Methods of teaching economic geology will, of course, vary with circumstances and with the personality of the instructor. Some confident spirits will be so sure of their own or adopted views that they will present these to their students with enthusiasm as the only true gospel. Others, more critical or less inspired, will have some doubts whether the whole truth is yet known. Shall these doubts be imparted to students or shall the teacher, for purposes of instruction, assume an appearance of that confidence which he can not feel?

The history of geology should, I think, make us cautious about accepting as final the theoretical views in vogue at the moment. By all means let us present to our students our own views as clearly as possible but let us also take pains to point out wherein these opinions differ from those held by other, and possibly most, economic geologists; and let us, furthermore, present as fairly as we can the theoretical ideas of those from whom we differ and whose ability and experience entitles them to a respectful hearing. The development of a questioning attitude in students, with the suggestion to them of the directions along which further investigation can be pursued, is believed to be more stimulating than the presentation of ostensibly complete and final theories. I believe, in short, in permitting our students to share our present doubts as well as to participate in our hopes for the future.

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THE ORIGIN OF ARTESIAN PRESSURE.[1]

W. L. RUSSELL.

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THE great Dakota artesian basin is the largest and most important in the United States and possibly in the world. It extends over eastern and western South Dakota and parts of Nebraska, North Dakota and adjoining states. In North and South Dakota alone about 15,000 wells have been drilled to the so-called Dakota sandstone, and the value of the water obtained from them is so great that the question of how long the supply will last is one of the most important economic problems of these states. Furthermore, as is shown elsewhere, the supposed artesian water circulation has had an important influence on the estimates of the oil possibilities of the region. No doubt also the solution of problems connected with the artesian pressures and artesian circulation in this basin will promote the solution of similar problems in other artesian basins.

ACKNOWLEDGMENTS.

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[1] Published by permission of the State Geological Survey of South Dakota. Taken from a dissertation submitted as partial requirement for the degree of Doctor of Philosophy at Yale University.

Dakota in 1924, 1925 and 1926. The writer is indebted to Professors Alan M. Bateman and Carl O. Dunbar for suggestions and criticism.

GENERAL STRATIGRAPHIC AND STRUCTURAL RELATIONS.

Stratigraphy.—A general conception of the stratigraphy and structure of the basin is necessary to a correct understanding of the artesian water problems. The chief source of the waters is the sandstone or series of sandstones immediately below the base of the Graneros shale. In the eastern part of the basin these sandstones are of Dakota age and rest upon the pre-Cambrian crystalline rocks. They are overlain by several hundred feet of soft clay shales, the lowest of which is called the Graneros. In central and western South Dakota this series of sandstones just below the base of the Graneros shale is underlain by a thick series of Paleozoic and Mesozoic rocks, in which there are several sandstone members. The Carboniferous Minnelusa sandstones are water bearing over a wide area. Owing to the nature of the unconformity at the base of the Cretaceous, the sandstone series just above the unconformity and just below the base of the Graneros shale rests on the eroded edges of the pre-Cretaceous strata. Because of the great pre-Cretaceous uplift of the eastern part of the basin, these basal Cretaceous rocks are underlain by older and older beds towards the east.

General Structure.—The general structure of the basin is simple. The sandstones beneath the base of the Graneros shale or its equivalents rise to the surface in the Black Hills and the Rocky Mountains on the west. In a narrow belt bordering these uplifts these water-bearing sandstones dip steeply towards the plains, and further eastwards they have a very gentle east dip for long distances. On the east side of the basin there is a very slight west and northwest dip to these strata. The outcrops of the aquifers on the west are about 2,000 feet or more higher than their exposures on the east, and there is a general eastward slope of the basin between.

THE AGE OF THE WATER-BEARING SANDSTONES.

Previous Ideas.—As will be shown later, the correlation of the aquifers has a very important bearing on the artesian water problems. The prevailing conception is that the upper water-bearing sandstone on the west side of the basin in the Black Hills is the same as the Dakota sandstones on the eastern side of the basin. If it can be shown that these two sandstones are not the same, the theories of artesian circulation in the basin must obviously be modified.

Two reasons may be assigned for correlating the sandstone just below the base of the Graneros shale in the Black Hills region with the Dakota sandstone of eastern South Dakota and Nebraska. The two sandstones occupy the same general position in the stratigraphic section—that is, they are both just below the base of the Graneros shale. Furthermore, it has been supposed that the dicotyledonous flora found in the so-called Dakota sandstone in the southeastern Black Hills region is of Dakota age.

Reasons for Conclusion of Pre-Dakota Age.—The reasons for this conclusion are elsewhere discussed in full by the writer.[2] A brief summary of these reasons is as follows:

1. Fossil plants collected by the writer from the Newcastle sandstone, which lies about 300 feet above the so-called Dakota sandstone of the Black Hills region, have been found by E. W. Berry to be older than the Dakota flora of the type locality in northeastern Nebraska. In regard to this Newcastle flora Berry[3] states: "The Newcastle sandstone plants are unquestionably older than the type Dakota, and correspond to the pre-Dakota flora of the Cheyenne sandstone of southern Kansas, and I presume with that found in the Mentor beds below the Dakota in central Kansas, although the Mentor flora has never been studied."

2. It appears that the supposed Dakota flora from the so-called Dakota sandstone in the southeastern Black Hills region would

[2] "Stratigraphy and Structures in Western South Dakota," South Dakota Geol. & Nat. Hist. Surv. Bull. 14, in press.

[3] E. W. Berry, Personal Communication, April, 1927.

not be classed as such if it were examined at the present time. When the original descriptions were made less was known about the subject than at present, and probably the correlations were not made so accurately or to such small subdivisions as is done now. Moreover, even in the original description Ward[4] recognized that this flora was older than the type Dakota, for he states that it corresponds to the "transition beds" of Kansas rather than to the plant-bearing horizons of the plains, and that it contains some of the flora of the Dakota group along with elements of the underlying floras.

3. The general paleogeographic relations during the deposition of the Dakota formation strongly support the idea that the so-called Dakota sandstone of the Black Hills region is older than the Dakota of the type locality. It was formerly supposed that both these sandstones were derived from the west, and while this conception prevailed it was natural to suppose that the two sandstones were the same. The data recently gathered by the writer indicate, however, that the sandstones were derived from a general easterly direction, and were laid down as an overlap along the shores of a sea which advanced from the west. If this is the case the Dakota of the type locality is younger than the so-called Dakota of the Black Hills region by the time it took the shore line to move from one locality to the other. Evidence for this conclusion is found in the direction of the cross-bedding and in the manner in which the top of the Dakota formation approaches the Greenhorn limestone towards the east. In the Black Hills region the oblique lamination in the Lakota, Fuson and the so-called Dakota sandstones have a marked northwesterly dip, suggesting a source to the southeast. In a few cases the cross-bedding is inclined towards the east, but when a great number of exposures are examined the predominance of the inclinations in a northwesterly direction is very striking. In northeastern Nebraska and western Iowa the oblique lamination of the Dakota sandstones has a prevailing west or southwest dip, indicating a source towards the east or northeast.

[4] L. F. Ward, "The Cretaceous Formations of the Black Hills as Indicated by the Fossil Plants," U. S. G. S. Annual Report, 1897–1898, part II, page 703.

The variation in intervals also suggests that the so-called Dakota of the Black Hills is older than the type Dakota. In the Black Hills region, the so-called Dakota formation is in places less than 75 feet thick. This is indicated by the fossil plants found by the writer in the northeastern Black Hills 75 feet below the base of the Graneros shale, which were found by F. H. Knowlton [5] to be unquestionably of Fuson age. Whether or not the strata between this horizon and the base of the Graneros shale is of Fuson age could not be determined, as no fossils could be found in them. The interval between the base of the Graneros shale and the base of the Greenhorn limestone is about 1,000 feet in this locality. In the type locality of the Dakota the relations are quite different. There the Dakota formation is 300 to 400 feet thick, and the interval between it and the base of the Greenhorn limestone is only 50 to 100 feet. It would appear, therefore, that the sandstones of Dakota age grow thicker and approach the base of the Greenhorn limestone towards the east, and that the formation was derived from a general easterly direction. Since in the vicinity of the type locality the formation is underlain by pre-Mesozoic rocks, and since it is overlain conformably by the marine Graneros shale, these relationships strongly suggest that the Dakota is an overlap deposit formed along the shores of a sea which advanced from the west. Taken by themselves, these data might perhaps be considered inconclusive, but it should be remembered that they are in perfect agreement with definite paleontologic evidence indicating that the so-called Dakota of the Black Hills is older than the Dakota of the type locality.

Definition of the Fall River Formation.—Since it now appears that the so-called Dakota of the Black Hills region is not of Dakota age, it is necessary to rename the formation. The term Fall River formation is therefore proposed to designate the sandstones and interbedded shales of post-Fuson age lying below the base of the Graneros shale. The type locality is at Evans Quarry on Fall River below Hot Springs, Fall River County, S. D.

The Lenticularity of the Aquifers.—The data gathered by the writer indicate that the old idea that the Dakota-Fall River

[5] Personal communication, Jan., 1926.

and Lakota sandstones are continuous over wide areas must be abandoned. By plotting a large number of sections through these formations in the Black Hills region and in the Dakota exposures in the vicinity of the type locality it was found that the sandstones are highly lenticular. By means of certain key horizons which may be correlated between the sections, it is possible to ascertain definitely that sandstone beds 50 to 100 feet in thickness are in a number of cases replaced by shale within distances of a mile or two. In fact, in areas where the exposures are continuous it is possible actually to observe these sandstones thinning out or being replaced by shale in less than a mile. As a result of these investigations, it is now clear that the type Dakota consists of a series of sandstone lenses embedded in shales, with the latter predominating. In the Black Hills region the formation between the base of the Lakota and the top of the Fall River consist of more or less lenticular sandstones with a considerable amount of shales. Shales and clays comprise a much larger portion of these strata than is generally supposed. The Fall River strata are, however, somewhat more persistent, and some of the sandstones contained in this formation may extend for ten miles or more. It is of course possible that certain sandstone strata in these formations persist for great distances, and it is impossible to prove that the sandstone lenses are not connected with one another. However, the general impression is that the aquifers are too lenticular to permit extensive artesian circulation.

PREVIOUS CONCEPTIONS REGARDING THE ARTESIAN WATERS.

Geologists have generally supposed that the artesian waters which are now being obtained in eastern South Dakota originally came from the Black Hills and the Rocky Mountains, where the Dakota sandstone, from which the waters are obtained, was formerly supposed to outcrop. According to the generally accepted explanation, the waters enter the sandstone at a high elevation on the flanks of the mountains to the west, and flow through it under the state towards the outlet at the lower eastern outcrop of the Dakota sandstone. The decrease of head towards

the east is supposed to be due to the friction or energy used up in forcing the water through the sandstone. The following statement of Darton [6] illustrates the prevailing ideas on the subject:

"Part of the rainfall and stream flow passes into the sandstones in their elevated outcrop zones in the Black Hills and flows eastwards through the interstices of the rock in its extension under the lower lands. Some of it finally escapes in springs in the outcrops in eastern South Dakota and Nebraska. In such water-bearing strata as the Dakota and underlying sandstones, which are overlain by a thick mass of impermeable deposits, the waters are under great pressure, for the intake zone has an altitude of about 4,000 feet and the region of outflow is only 1,000 to 1,200 feet above sea level. The evidence of this pressure, as found in many wells in eastern South Dakota, is conclusive that the water flows underground for many hundreds of miles."

In discussing this problem, Gould [7] says:

"The origin of the Dakota water is a mooted question. Some authorities look for the source of the supply in the Dakota outcrops along the eastern slope of the Rocky Mountains. They would consider the water artesian and argue that deep wells over the central part of the state should tap this supply and furnish artesian water. This theory seems quite tenable, but in practice it remains to be proven. That it will not account for all the Dakota springs is illustrated by the fact that in several instances some of the best springs issue from isolated buttes, cut off by erosion from the main body of the Dakota. It seems more probable that much of the water supply comes from local rainfall, although until the artesian theory is either proven or disproven we may continue to look further away for a part of the supply."

Four different arguments have been cited to prove the existence of the artesian circulation. It was formerly supposed that the Dakota sandstone was a porous and persistent stratum, outcropping in high levels in the Black Hills and at lower elevations to the east. Hence it was natural to suppose that the waters ran through the formation from the higher outcrop to the lower. The general eastward decrease in the head or altitude to which the water will rise has also been cited as proof that the waters

[6] Darton, N. H., "The Geology and Underground Waters of South Dakota," U. S. G. S. Water Supply Paper 227, p. 60.

[7] Gould, C. N., "Some Phases of the Dakota Cretaceous of Nebraska," *Am. Jour. of Science*, 4th Ser., vol. 9, p. 430, 1900.

are flowing from west to east. It has also been claimed by Darton [8] that the streams in the Black Hills region diminish in volume where they flow over the so-called Dakota sandstone, and it was supposed that the water entered the sandstone at these points and flowed under the plains to the east. The springs issuing from the Dakota sandstone in its outcrops in eastern Nebraska and adjacent territory have also been cited as a proof of this artesian circulation, for it was supposed that they were formed by the waters which flowed under the plains from the Black Hills and Rocky Mountains.

OBJECTIONS TO THE THEORY OF ARTESIAN CIRCULATION.

The Lenticularity of the Aquifers.—So long as the idea prevailed that the Dakota sandstone was continuous and pervious across the basin, it was inevitable that the theory of artesian circulation should be generally accepted. However, as has been previously shown, it now appears that the sandstones outcropping on the east and west sides of the basin are not the same, and that the Dakota sandstone in the eastern part of the basin is extremely lenticular, consisting of sandstone lenses embedded in clays. Of course it is impossible to tell from the outcrops and well logs whether or not the sand lenses are connected. However, the known facts certainly do not conflict with the supposition that the Dakota sandstones are too lenticular to permit the migration of water for long distances. If a migration across the basin through the sandstones immediately below the base of the Graneros shale does occur, the waters must flow several hundred feet across the strata.

The Separation of the Upper and Lower Waters.—There are, however, two reasons for supposing that such migration transverse to the strata does not occur. In the first place, it is found that the head of the water in the lower beds all over eastern South Dakota and parts of eastern North Dakota is greater than that of the water in the upper Dakota sandstones. If there was

[8] Darton, N. H. "The Geology and Water Resources of the Northern Black Hills and Adjoining Regions," U. S. G. S. Prof. Paper 65, p. 84, 1909.

free communication between the upper and lower beds the head would have become practically the same in both. Furthermore, the chemical character of water in the upper and lower water-bearing formations is quite different in Minnesota and North and South Dakota. According to Hale, Meinzer and Fuller,[9] the upper waters, just below the base of the Graneros shale, contain much sodium chloride, and are relatively soft, while the lower waters are hard and contain but little sodium chloride and much calcium and magnesium carbonate. This condition apparently indicates that the waters from the two zones do not mix extensively, as they would be expected to do if there were migration from the lower zone to the upper.

The Lateral Variation in the Character of the Waters.—The variation in the chemical character of the waters from east to west is another indication that there is no general movement from west to east. The chemical character of the water varies considerably from place to place, as is indicated not only by the analyses, but by the taste. In the vicinity of Aberdeen and Redfield, for example, the taste of the water is very strong. Further west, in the vicinity of Gettysburg, there is but little taste to it. Still further west, along the Missouri River, the water is distinctly salty and contains gas in solution. The eastern boundary of the area containing this salty water runs nearly north and south. It is believed to occur in the sandstones immediately below the Graneros shale over most of the northwestern part of the state, for it was found in a well on Snake Creek, in northern Haakon County, and in a well at Zeal in eastern Meade County. Fig. 1 indicates the area in which the salty waters with gas are found in these sandstones. If the waters from the Black Hills have moved through the sandstone below the Graneros shale from west to east, such a belt of salty waters could not occur. One would expect that they would either have been swept away long ago, or that the water to the east would also be salty. At any rate, it is certain that fresh water cannot flow through a sand-

[9] Hale, C. W., Meinzer, O. E., and Fuller, M. L., "The Geology and Underground Waters of Southern Minnesota," U. S. G. S. Water Supply Paper 256, 1911, p. 69.

stone still containing salty water, and remain fresh. It is of course conceivable that the water flows through some lower horizon, such as the base of the Cretaceous, or the Minnelusa, and then rises into the Dakota, but there is no definite evidence in favor of this.

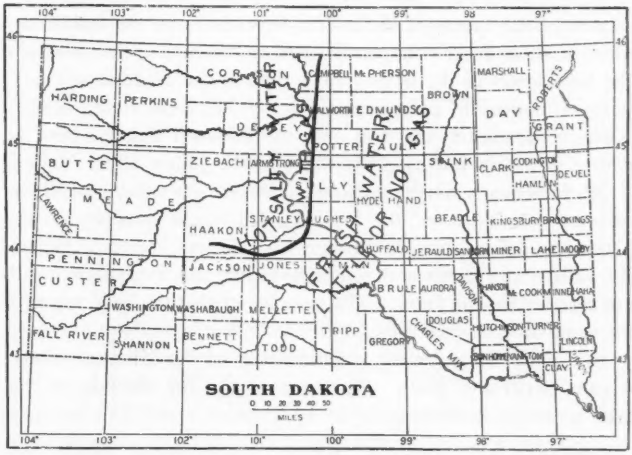

FIG. 1. Map showing area in South Dakota underlain by hot salty water with gas.

The Variations in Head.—Moreover, when the distribution of the pressures in the Dakota sandstone is examined critically, some conditions are disclosed which are difficult to explain by the old theory that the decrease in head towards the east is due to the work done in forcing the water through the sandstone. If the water came from the Black Hills, the lines through points of equal head should curve around them to some extent, and this should be apparent even in eastern North Dakota, South Dakota and Nebraska. However, nothing of this nature may be detected in Darton's map.[10] Furthermore, if the old hydraulic theory

[10] Darton, N. H., "The Geology and Underground Waters of South Dakota," p. 61.

is correct, the lines of equal head should also curve around the areas where the sandstone outcrops at a low level, as for example in the southeastern corner of South Dakota, while the head should be greater where the sandstone thins out and disappears against the pre-Cambrian rocks, without outcropping towards the east. As a matter of fact, however, the map shows no particular influence of the nature of the eastern termination of the sandstone. One of the most striking peculiarities of this map is that it shows the head lower in the large valleys than in the adjacent uplands. This is noticeable in the case of the Missouri River Valley where it runs nearly east and west, the Missouri Valley where its course is nearly north and south, and the James River Valley, which runs nearly north and south. Where the valley extends in an east-west direction, the loss of head might possibly be explained as due to leakage through the overlying strata. Where the valleys run north and south, however, this explanation will not account for the facts. The map shows that the head remains the same or even increases slightly for about 25 to 50 miles east of the James River Valley and the valley of the Missouri where it runs north and south, whereas normally the altitude of the head increases towards the west at a rate of about four feet per mile. If there is any leakage from the Dakota sandstone through the overlying shales it may be supposed to be much greater in the valleys where the covering is thin. It is not possible, however, for leakage from the valleys to produce an arrangement of pressures such as is actually found. If the waters are flowing from west to east, there must be a steady decrease in head from west to east. If the altitude of head should remain the same for an east-west distance of 25 to 50 miles, as it does just east of the Missouri and James valleys, there could be no flowage of the waters across these areas, and consequently no flow either to the east or west. Moreover, in some places the pressure is shown on Darton's map as actually increasing towards the east just east of these valleys. If the waters were moving from west to east, as Darton supposed, a change in the rate of decrease of head towards the east might be expected if there was a great leakage,

but the pressure could not increase towards the east, or remain the same for an east-west distance of 25 to 50 miles. It is of course, possible that there is leakage around the casings of the wells when they are measured under the great pressures occurring in the valleys, and that this makes the pressure seem greater in the uplands. However, it does not seem likely that a mistake would be made in all, or nearly all, of the measurements made in the valleys. It is also true that there are generally more wells in

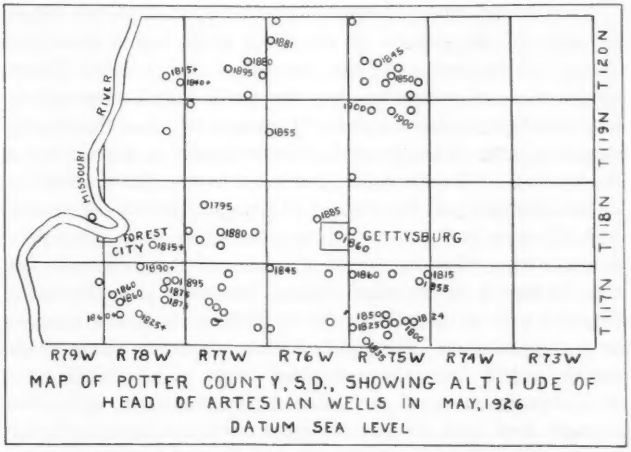

FIG. 2. Map showing variation in the head of artesian water in Western Potter County, S. Dakota.

the valleys than in the uplands, where the flow is less and the sand is deeper, and it might be supposed that the greater number of wells in the valleys caused the lowering in the altitude of the head in them. Nevertheless, one would suppose that a number of the measurements were taken before the production of wells was large enough to effect a marked lowering of the head. Moreover, the wells at Gettysburg and at Cheyenne Agency indicate that this is not the explanation of the conditions in that part of the

Missouri Valley, for the well at Cheyenne Agency was drilled when there were very few wells in the region. Fig. 2 shows the head of water in the artesian wells in this region in May, 1926. It is evident that there is no general decrease in the head towards the east, as there should be if there is an eastward migration. Of course this does not mean that such an eastward decrease did not exist before the head was modified by the extraction of the water, but at least it proves that there is at present no general eastward migration in this area.

The Sinking of the Streams.—Darton [11] states that the volume of many of the streams in the Black Hills region diminishes greatly where they cross the outcrop of the so-called Dakota sandstones, and concludes that the water enters these porous beds and flows under the state. It is certainly true that the apparent volumes of the streams decrease greatly in the vicinity of the outcrops of this formation, but it is also true that the outcrops in the valleys of all but two of the larger streams are covered with alluvium and gravel in these localities and for many miles downstream. The outcrop of the base of the Graneros shale and the top of the so-called Dakota formation generally lies at the outer edge of the hills, where the gradients of the streams become comparatively slight, and where consequently the sand and gravel washed from the mountains begin to accumulate. As these deposits are very permeable, large volumes of water flow through them, and the streams seem much smaller than higher up where they flow over bed rock. Some of the smaller streams undoubtedly do sink into the Fall River (Dakota) strata, which are very porous. However, these beds are in contact with the alluvium at much lower levels in the valleys of the larger creeks only a few miles away, and it is therefore possible that the waters merely issue at these points. It should be understood that, while the general character of the sandstones is such that it is doubtful if they are sufficiently persistent to carry waters under the whole state, they are undoubtedly extensive enough to permit a passage for several miles, and probably in some cases for five or ten miles.

[11] Darton, N. H., *op. cit.*, p. 61.

As already mentioned, two larger streams, Fall River and Cheyenne River, flow over bed rock where they cross the Fall River strata, and therefore any diminution in their volumes at these points cannot be explained as due to seepage through the alluvium. However, the Fall River strata are exposed on Fall River outcrop only a mile away on the Cheyenne River at an elevation of 100 feet or more lower. Hence the water entering them on Fall River could easily issue at the lower outcrop. The other stream which flows over bed rock at or below the exposures of the Fall River formation is the Cheyenne River. It is stated by Darton [12] that a large portion of the water appears to sink into the Fall River (so-called Dakota) and Lakota strata in the vicinity of Cheyenne Falls. However, as these same beds are in contact with the alluvium at considerably lower elevations three or four miles northeast, it is possible that the waters issue here instead of flowing under the state. It appears, therefore, that the evidence as to sinking and extensive migration is rather inconclusive.

The Evidence of the Springs.—It is also impossible to prove that the springs in the eastern outcrops of the Dakota sandstone are produced by the issuing of waters which have flowed underground from the Black Hills or Rocky Mountains. It is possible that in most cases the water is simply rain water which has entered the sandstone in the immediate vicinity. The occurrence of springs in the isolated buttes is a proof that some of them are formed in this manner. A few of these springs in Nebraska [13] are salty, which is an indication that they are not due to rain water. However, this is also an indication that they are not formed by artesian circulation, for the waters which have been supposed to enter sandstones in the Black Hills are fresh, and the waters in the Dakota sandstone in the areas of the salt springs are also generally fresh.

[12] Darton, N. H., "The Geology and Water Resources of the Southern Half of the Black Hills and Adjoining Regions in South Dakota and Wyoming," U. S. G. S. 21st Ann. Rept., pt. IV., p. 575.

[13] Gould, C. E., "The Dakota Cretaceous of Kansas and Nebraska," *Trans. Kansas Acad. Science*, vol. 17, p. 153.

SUBSIDENCE OF THE SURFACE WITH WATER WITHDRAWAL.

The Loss of Head.—It has been generally recognized that as the artesian wells produce, the pressure or head gradually decreases. Meinzer and Hard [14] state that in the vicinity of the Edgely Quadrangle, North Dakota, this loss of head diminishes to the west from about 300 feet to about 150 feet in 15 miles, and gradually disappears further west. The same writers estimate that the amount of inflow or recharge from the west is about 400 to 500 gallons per minute for each east-west row of townships. As the waters have been withdrawn at a rate of about 3,000 gallons per minute, they conclude that the sandstone has been compressed by an amount equal to the excess of the artesian flow over the recharge. According to their calculations, this would amount to a layer of water 4.4 inches deep over the eighteen townships. In discussing this question, they state:

> It has long been known that at least slight compression of water-bearing beds may result from additional loads at the surface, such as tides or even railroad trains. It is also well known that, other things being equal, the porosity of rocks decreases with depth because of increase in the weight they must support. It seems within the range of possibility, therefore, that the upper part of the Dakota sandstone, which is being here considered, should have undergone a compression of a few inches—probably less than 1 per cent.—as a result of the release of the expansive forces of artesian pressure and a consequent increase of about one half in the load upon the formation.

In South Dakota the loss of head varies greatly from place to place, being in general largest where most water has been used. In some areas it appears to be 200 feet or more, while in others it is only about 50 feet. It is not at present possible to determine the original variations in the head, owing to the modifications which have been produced by the withdrawal of the water. Hence such data must be sought in Darton's [15] map, which shows lines drawn through points of equal head. The present varia-

[14] "The Artesian-Water Supply of the Dakota Sandstone in North Dakota, with Special Reference to the Edgely Quadrangle," U. S. G. S. Water Supply Paper 520-E, 1925.

[15] Darton, N. H., *op. cit.*, p. 61.

tion in the altitude of head in western Potter County is shown in Fig. 2. It is evident that the altitude of head varies from about 1,800 feet to about 1,900 feet. As the original head was about 1,975 feet, the loss is from 75 to 175 feet.

Comparison of Artesian Circulation and Expansion with the Volume Extracted.—A brief calculation shows that the artesian waters of eastern South Dakota must have been expelled from the sand by a subsidence of the overlying strata, as the pressure on the water was relieved. As will be shown presently, it is very unlikely that there is any appreciable movement of the water from the west, but even if a migration as extensive as that assumed by Meinzer and Hard was taking place, it could not have supplied a sufficient volume of water. If about 500 gallons per minute flowed across each township, the amount flowing across the state of South Dakota would be about 16,000 gallons per minute. As there have been about 10,000 wells in the eastern part of the state producing an average of about ten gallons per minute, the total amount of water taken out of the sandstone has been about 100,000 gallons per minute. The expansion of the water and of the sand grains due to the release of the pressure is also entirely inadequate to account for the volume used. According to Mr. George Norbeck,[16] there are 10,000 or 12,000 wells in South Dakota east of the Missouri River, and several hundred west of the Missouri River, chiefly in Lyman, Stanley and Gregory Counties. When first drilled, these wells averaged about 15 to 20 gallons per minute, though the average flow is now probably about 3 or 4 gallons per minute. A number of towns and cities also use water from the Dakota sandstone, the amount being about 100 gallons per day per capita. From these figures, and from the population of the towns involved, the total amount used by the cities and villages may be estimated at 40,000,000,000 gallons. On the basis of an average yield of 10 gallons per well per minute from 10,000 wells, and an average age of 15 years, the amount of water produced from the farm wells is about 800,000,000,000 gallons. As there are no gas pools to expand

[16] Personal Communication, Dec., 1926.

and replace the water as it is withdrawn, it is difficult to explain how the space in the sand could have been filled without a subsidence of the surface. It has already been shown that the rate of flow from the west, even if it occurs, is entirely inadequate, and it appears that the expansion of the sand grains and the water in the sandstone is also far too slight to have an important effect. The area underlain by the Dakota sandstone in eastern South Dakota is roughly 30,000 square miles, and the average thickness of the bed from which the water is produced may be estimated at 50 feet. The cubic compressibility of quartz at 0 megebars is 2.7 parts in 10,000,000 per megabar, according to Leith,[17] and the proportion by which the volume of water is increased when the pressure on it is increased by one atmosphere is 0.0000489. As already stated, the loss of head in eastern South Dakota varies greatly, being 200 feet where the wells have been drilled close together and much water used, and only 50 or 75 feet where but little water has been extracted. Since there are considerable areas in which there are no wells at all, the average lowering of head is probably about 100 feet. From these figures the volume displaced by the expansion of the quartz sand grains by the release of pressure may be calculated to be about 180,000,000 gallons, while the expansion of the water is equal to about 16,000,000,000 gallons.

The Amount of the Subsidence.—It is evident, therefore, that there must have been a compression of the sandstone and a subsidence of the surface. The volume of about 840,000,000,000 gallons, if spread uniformly over the artesian area of eastern South Dakota, would make a layer nearly two inches thick. Hence the average amount of subsidence must be nearly two inches, though it is doubtless greater where larger volumes of water have been used. Some idea of the amount of subsidence near the largest cities may be obtained from the following calculation: The average population of Aberdeen during the last 30 years has been about 10,000. From the manner in which the loss of head varies in the regions where it is known, it seem likely that the average

[17] Leith, C. K., "Structural Geology," p. 307, 1923.

loss due to the water used by the city is not more than 150 feet over an area of 1,600 square miles. It is probably greater near the town, diminishing away from it and gradually disappearing. No doubt the head is lowered over an area much greater than 1,600 square miles, but this is due to the wells outside the town, which must be excluded from the calculation. Assuming as before a per capita consumption of 100 gallons per day, the total amount of water extracted would amount to about 10,000,000,000 gallons. The expansion of the water is almost negligible, for according to the assumption that the producing formation is 50 feet thick it would amount to only 150,000,000 gallons. The 10,000,000,000 gallons, if spread over an area of 1,600 square miles, would make a layer slightly less than one half an inch thick, and according to the assumption made, this should be the average amount of subsidence in the 1,600 square miles due solely to the water used by the city.

The Necessity for Subsidence.—Perhaps the statement that the withdrawal of such great quantities of water causes a compression of the sandstone and subsidence of the surface needs some explanation. As Meinzer has already noted, the weight of the sediments overlying the sandstone is supported in part by the sandstone itself, in part by the head or hydrostatic pressure of the water, which presses against the roof of the sand body and in this manner upholds part of the weight. As the head decreases, it is not capable of supporting as much weight, and consequently more of it must be borne by the sandstone. In some places the actual load on the sandstone has been increased in this manner by one fifth. As all substances are compressible, though in some cases to an infinitesimal degree, it is inevitable that this increase in pressure produces some yielding in the sandstone. The roof of the sandstone must sink an amount equivalent to the yielding, and the overlying materials also settle. As the sandstone is compressed its volume decreases, and as there is no longer so much room for the contained water, some of it is expelled through the wells.

It is evident that the space formerly occupied by the artesian

waters taken from the sandstones must be filled in some manner. If it were not filled, there would be a vacuum in the sandstones, and the fact that water is still being expelled from the sandstones under considerable pressure shows that this is not the case. The only methods by which the space formerly occupied by the waters could be refilled are (1) by the accession of an equivalent volume of water, (2) by the expansion of gas to fill the space, (3) by the expansion of the quartz grains and water as the hydrostatic pressure is relieved, and (4) by the compression of the sandstone and consequent subsidence of the overlying strata. It has already been shown that the first three methods are entirely inadequate. Hence there must have been a compression of the sandstone approximately equal to the volume of the artesian water that has been produced.

The Manner of Compression.—The Dakota sandstone in its exposures in northeastern Nebraska and western Iowa is for the most part exceedingly soft and very feebly consolidated. Some samples show a porosity of over 40 per cent., according to measurements made by the writer, and many pieces which have been dried crumble to a loose sand in a few seconds on being immersed in water. It might be supposed, therefore, that, when the load on the sandstone is increased by the removal of the hydrostatic pressure, the rock would be crushed and the sand grains would arrange themselves so as to occupy a smaller volume. It is possible that something of this nature does take place, but it is difficult to accept this explanation of the compression of the sandstone, for it is known that the load was once greater than it is at present. Hundreds of feet of Cretaceous and Tertiary rocks, now entirely eroded away, once covered the region, and the load of these strata must have been greater than that produced by the loss of head. Hence if an additional load of 100 or 150 lbs. per square inch were sufficient to crush the sandstone, it would have already yielded, and, once the grains had rearranged themselves, they could not resume their former positions upon the release of the load by erosion. It seems likely, therefore, that the compression of the sandstone was of an elastic nature, so that the rock

could resume its former volume as the overlying strata were worn away. The pressure of the superincumbent material is concentrated on the minute areas where the sand grains are in contact with one another, and it is possible that the yielding was at these points.

THE ORIGIN OF THE ARTESIAN WATERS.

From the foregoing discussion it is evident that there is no definite proof of an artesian circulation in the Dakota strata, and that, even if such a migration does occur, it is not capable of explaining the distribution of waters of different chemical types, or some of the variations in the artesian pressure. Furthermore, the volumes of water extracted by the artesian wells are so enormous that they cannot be accounted for by the hydraulic movement of the waters. In view of these circumstances, the writer proposes a new theory which appears to fit the facts better than the old one.

There is no difficulty in accounting for the presence of great quantities of water in the artesian strata, for all of the sandstones were laid down under water, and must have been saturated with it since they were formed. As most of the Dakota strata are of terrestrial origin, this connate water was doubtless originally fresh. Some of the upper sandstone members were deposited in salt or brackish water, and it might be supposed that, if there were no artesian circulation, they would now contain salt water. This suggests an explanation of the general difference in the chemical composition of the waters from the upper and lower portions of the formation. As is shown elsewhere,[18] the upper beds of the Dakota sandstone are not everywhere of the same age. They were formed as overlaps or oblique transitions along the shores of an advancing sea, and the topmost strata are much younger towards the east. Hence the salty waters are not confined to the same horizon, but occur in a narrow zone which cuts across the strata obliquely. The hard waters are encountered in the lower strata which were formed on land. The salty waters, on the other hand, are found only in the upper layers, which were

[18] Russell, W. L., *op. cit.*

either laid down in salt or brackish water, or are closely associated with the lower part of the Graneros shale, which is of marine origin. It appears, therefore, that the difference in the chemical character of the waters is due to the conditions of formation of the strata in which they occur. The hard waters, which contain relatively large quantities of calcium sulphate, were entombed in terrestrial beds. Where these lower rocks are exposed they are found to contain numerous scattered crystals of gypsum, and it is probable that the calcium sulphate of the hard waters came partly from the solution of similar gypsum crystals. The comparatively large quantities of sodium chloride contained in the upper waters probably came from the brackish water in which these topmost Dakota strata were in many places deposited, or else were forced out of the marine Graneros shales as they were compacted. The belt of salty and gassy waters in the northwestern part of the state is apparently related to the small-scale, faulted structures which are extensively developed in this region. At all events, where the Dakota has been tested in the region in which these faulted structures are known to occur it has always been found to contain the salty water and gas, as for example along the Missouri River in the vicinity of the Cheyenne Agency. On the other hand, where it has been penetrated in areas where the faulted structures are known not to occur, as at Connata, it has been found to contain fresh water. This suggests that gas and salt water may have been forced out of the overlying Graneros shale into the sandstones during the folding and faulting. Possibly the faults served as passageways for the migration of these fluids.

THE ORIGIN OF THE ARTESIAN PRESSURE BY COMPRESSION

It has already been shown that there must be a subsidence of the surface in eastern South Dakota in order to account for the enormous volumes of water that have been produced from the artesian wells. Since the surface subsides as the pressure of the artesian water is reduced, it is evident that this pressure has borne part of the load due to the weight of the overlying mate-

rials, or in other words that this load exerts a pressure on the artesian waters as well as on the sandstones containing them.

The Dakota sandstones probably consist partly, if not entirely, of disconnected lenses. There are a number of reasons for supposing that pressures as great as those of the artesian waters of the Dakota sandstone could arise in such lenses simply by the compaction of the sediments and the weight of the overlying strata. The sands when first deposited were doubtless poorly compacted. As the load on them increases they tend to become more compact, and their volume is also decreased by the elastic yielding of the grains at their points of contact, and eventually by solution at these points, and deposition on the surfaces which are not under such great pressure. The compaction of the shales and clays is even greater than that of the sandstones, for as Hedberg [19] has shown, the porosity of the argillaceous rocks decreases steadily as they are buried to greater and greater depths. As their volume decreases, the water in the interstices between the particles is driven out under great pressures. These connate waters from the shales and clays closely associated with the sand lenses are expelled into the porous sands until the rise of the hydrostatic pressure of the waters in them prevents this movement. As water is comparatively incompressible, the access of small amounts of it will cause a great rise in the pressure. If the escape of the waters from the compacting sediments was absolutely impossible, the hydrostatic pressure might approach the pressure of the overlying column of rock. It is, however, certain that there is an appreciable, though exceedingly slow, percolation through the shales and clays. If this were not the case it would be impossible for thick bodies of shale to become compacted. This gradual filtration upward cannot begin until the pressure or head is equal to the hydrostatic pressure of a column of water reaching from the sand lens to the water table at the surface. Owing to the enormous resistance to flow through the shales, it is probable that the percolation through them would not become appreciable until the hydrostatic pressure was much

[19] Hedberg, H. D., "The Effect of Gravitational Compaction on the Structure of Sedimentary Rocks," *Bull. Am. Assoc. Pet. Geol.*, vol. 10, pp. 1035–1072, 1926.

greater than that necessary to send a column of water to the surface. Hence if wells were drilled to the lens, the water would flow out at the surface under considerable head.

ORIGIN OF THE APPARENT HYDRAULIC GRADIENT BY DIFFERENTIAL UPLIFT.

The differential uplift of a region underlain by a series of disconnected sand lenses of the type just described would inevitably produce an apparent hydraulic gradient. As the water could not flow along the strata down the hydraulic gradient from one lens to another, the only way the pressure due to compaction could be relieved is by slow percolation to the surface, as already described. The altitude of the head would therefore tend to become equal to the altitude of the water table plus the amount necessary to force the waters expelled from the compacting sediments through the overlying impervious shales. Hence the hydraulic gradient should correspond roughly to the general slope of the surface, and should reflect in modified form the larger topographic features.

It is evident that this theory is capable of explaining not only the hydraulic gradient in South Dakota, but also the anomalous variations in pressure which could not be accounted for by the old hypothesis of artesian circulation. There is no doubt that the western part of the state has been uplifted during comparatively recent times, probably in the Pleistocene. The altitude of the head of the artesian waters is at present highest in the areas in the western part of the state, which has been uplifted most, and lowest in the eastern part, which has been uplifted least. The lines through points of equal head do not course around the Black Hills, but follow approximately the general contours of the surface. This is what would be expected by the writer's theory of compaction and differential uplift, but contrary to what would occur if the waters were flowing from the Black Hills through the sandstones. The lowering of the head in the vicinity of the large valleys is also a logical consequence of the assumption that the pressure is produced by compression and compaction.

The weight of the overlying materials is obviously greater under the ridges than in the large valleys, and hence the compression of the sandstones and the hydrostatic pressure of their contained waters should also be greater. In addition, the thickness of impervious shales through which the waters expelled from the compacting sediments must flow is greater under the divides than under the valleys. Hence more hydrostatic pressure would have to accumulate before these waters could escape. It is also possible that the greater altitude of head beneath the divides is produced by differential uplift. It is shown elsewhere [20] that the major divides in the portion of the South Dakota between the Black Hills and the Missouri River are located along anticlines which were formed in the middle or late Tertiary or the Pleistocene. It is conceivable that the divides between the Missouri River and the James River and the divide east of the James River are also located along uplifts.

The conditions which the old theory of hydraulic migration is unable to explain are the presence of waters of different chemical characteristics in the upper and lower parts of the artesian sandstones, the occurrence of waters of different chemical characters in different regions, the greater head of the waters in the lower strata, the lack of any close relation between the contours of head and the supposed intake and outlet areas of the artesian waters, and the fact that the head is as low in the north-south valleys as on the ridges to the east. The theory that the artesian pressures arose from compression and compaction is able to account in a satisfactory manner for the variations in the character of the waters from place to place, for, since no migration is assumed, these differences may arise from original variations in the character of the sediments or of the waters contained in them. According to the compression theory it is to be expected that the head would be greater in the deeper strata, where the weight of the overlying materials is greater, and that the contour lines of head would tend to follow the surface contours, for when the ground is uplifted the altitude of the head of the water in the

[20] Russell, W. L., *op. cit.*

sand lenses is raised also. The increase in head on the ridges and divides is also easily explained as being due to the greater pressure of the overlying strata in these localities.

APPLICATION TO OTHER ARTESIAN AREAS.

The foregoing theory may be applied to all areas in which the formations are soft and the water bearing formation lies in disconnected lenses. Where the sandstone is continuous for great distances there would of course be an artesian circulation like that which was formerly supposed to occur in the Dakota sandstones. Where, on the other hand, the sandstones are lenticular but the formations comparatively rigid, it is to be expected that the weight of the overburden would cause a much smaller increase in pressure than would be the case in soft strata like those of the Dakota series. If the yielding of the soft, porous sandstones is at the points where the grains are in contact, this compression should be greatly reduced if the sand grains are cemented, for then the stresses are not concentrated and magnified in certain minute areas. Moreover, the water is in some cases partially absorbed into formations which were dry or partly saturated with water when deposited. However, since even the hard sandstones are slightly compressible, and since there is usually still some water to be expelled from the associated shales, a general increase in the pressure with depth is to be expected.

These considerations appear to be in accord with the facts, for it is well known that in the older, more rigid rocks the pressure tends to increase with depth, but is generally not sufficient to send a column of water to the surface. Aside from the continuous sandstones in which true artesian circulation takes place, flowing wells should be more common in the softer strata of later origin, and this, too, appears to be the case.

THE BEARING OF THE THEORY ON OIL AND GAS ACCUMULATION.

It goes without saying that the theories just set forth have an important bearing on the accumulation and migration of oil and gas, not only in the " Dakota " basin, but in other areas where

the conditions are similar. This phase of the subject is, however, outside the scope of the present paper, and is discussed in full by the writer[21] elsewhere.

THE FUTURE SUPPLY OF ARTESIAN WATER.

If the theory just proposed is the correct explanation of the origin of the artesian waters and their pressures, then the future supply of water is much less than is generally supposed. The hypothesis of hydraulic migration postulates that there is a steady influx of water amounting to perhaps 400 to 500 gallons or more per minute for each tier of townships. While this is much less than has been used, it could be made to suffice if carefully conserved. If this is the case the reserves of water are inexhaustible. According to the compression-compaction theory, on the other hand, there is only a definite amount of water obtainable, and the supply is never renewed. Hence the head will steadily decrease until the water level reaches the sandstones themselves. As the lowering of head is on the average in the neighborhood of four feet a year, and as the depth of the wells is generally greater than 400 feet, and is in many places over 1,000 feet, there is no prospect of an immediate exhaustion of the supply. However, the cost per gallon will steadily increase as the depth from which it is necessary to pump the water grows greater. When the water level reaches the producing sandstones the amount remaining in them will be much greater than the volume previously extracted. This will doubtless seep slowly into the wells, but because of the small yield and considerable depths it may not be profitable to pump it out.

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[21] W. L. Russell, *op. cit.*

MICROSCOPICAL INTERPRETATION OF FOLDED STRUCTURES.

L. C. GRATON AND S. C. DAVIDSON.

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INTRODUCTORY STATEMENT.

In many ore deposits formed by replacement, definite structural characteristics of the original rock, such as foliation or bedding, survive the replacement process and are "inherited" by the ore.[1] By means of these structural features thus preserved, the structural history of the mass may be more or less completely recorded and revealed. When such initial rock structures are preserved in the ore in plainly undisturbed condition, surer proof is afforded than can be had in any other way that both the ore and the orig-

[1] It is our belief that numerous occurrences described in the literature as examples of regionally metamorphosed ore deposits are in reality cases of replacement, by the ore minerals, of rock which had previously undergone all or most of the observed deformation. It is not intended to set forth in this paper the foundations for that belief—and only incidentally do parts of the present discussion bear upon this question.

inal rock have had a structurally static or uneventful history. On the other hand, by means of these original structures there may be plainly revealed brecciation, faulting or folding, which might be difficult or impossible of detection if the original rock were "structureless," *i.e.,* essentially uniform in all directions, like a granite for example. In this second type of case, the *rock* has obviously suffered the deformation which is evident; but of course the *ore,* being of later age, may or may not have suffered all or any of that deformation—it may merely have replaced rock already so deformed.

For a variety of reasons too obvious to need enumeration here, it is generally important for the geologist to determine whether the exhibited deformation took place before or after the introduction of the ore materials.

Where brecciation or faulting are involved, definite fractures permitted the individual rock pieces to move relative to one another. If this fracturing and disturbance preceded ore deposition, the cracks and other voids are likely to be found filled, or partly filled, by minerals deposited from the ore-carrying solutions, since open-space filling and rock-replacement can readily go on side by side and simultaneously from the same solution. Under such circumstances there can be little [2] uncertainty as to the relative dating of deformation and ore deposition. On the other hand, if brecciation or faulting followed ore deposition, then the fractures and inter-fragment spaces will be empty.[3]

But where the deformation consists of folding, obvious fractures may not, and often do not, result; for here, the dislocation is general and distributed rather than localized and concentrated as in faulting or brecciation. In these cases of folding, therefore, it is plainly impossible to employ the criterion of relative age of deformation and ore deposition mentioned just above as commonly useful in the case of fracture-producing deformation, viz., whether the fractures and voids are on the one hand empty, or on the other hand are more or less filled by minerals of the

[2] But see also later statements regarding preservation of *unfilled* cracks.

[3] Unless filled by later mineralization, which ordinarily will differ in mineral character and thus be distinguishable from the major or earlier ore mineralization.

ore-forming period. Some other criteria must be depended upon if the problem is to be solved. It is to the determination of cases in which folding is a dominating structural feature that this paper is directed.

FIELD EVIDENCE.

In certain instances, the general structural history of the region may be so evident and the time of ore introduction so clearly indicated that a conclusion as to relative dating of folding and ore formation may safely be drawn. In other instances, there may be such significant positional coincidence of ore and folds [4] as to make it almost or quite certain that the folds occurred first and served to localize the ensuing ore deposition. The opposite case, however, in which ore shows no especial dependence on folding, may not be taken, without further evidence, as proof that folding followed ore deposition. In still other cases, the ore may clearly replace minerals, like chlorite or mica, which by their nature, habit, arrangement and general distribution show that they were developed independent of the ore mineralization and in consequence of the stresses which produced folding. Still another kind of evidence would be the presence in the ore of delicate structures like clusters of fragile crystals in a vug, which could hardly have survived the severe deforming stresses of folding. Finally, the ore of the folded structure may plainly be continuous with similar material formed in or along off-shooting fractures which were obviously formed by the folding, thus making the ore plainly post-fold. Absence of the features mentioned in the three sentences just preceding might indicate but could not prove that the ore was earlier than folding. On the contrary, obvious fracturing of the ore in the most folded places, with little or no fracturing elsewhere, would indicate [5] post-ore folding. But ab-

[4] The "saddle reefs" belong in such a category, although these are regarded as open space fillings mainly, and only in subordinate degree due to replacement. Another type of case in which ore deposition clearly followed folding is that in which the thickened crests have been replaced by ore while the thinned sides or limbs of the fold are only slightly mineralized.

[5] Subject to reservations later mentioned.

sence of such fracturing would not prove the ore to be younger than the folding.

MICROSCOPICAL EVIDENCES OF STRAIN.

Any or all the foregoing kinds of evidence may be developed on a scale readily recognized in the course of ordinary field study. But in many cases, external, general, or large-scale evidence of the kinds just enumerated may be lacking or unconvincing. The consequence is that in the interpretation of these ore-bearing folds, recourse is often had to microscopical study in the hope that evidence impossible of detection by the unaided eye may be found to show whether or not the ore was present when the folding took place. In largest measure the conclusion yielded by such microscopical examination hinges on whether or not evidences of strain are detected in the ore.

The expected evidence that the ore had already been formed before the folding occurred would comprise one or more of the following: (1) Cracks traversing or separating the ore grains and themselves devoid of ore-mineral filling (*i.e.*, merely a development on a microscopical scale of one kind of evidence already cited among the field criteria), (2) "pressure twinning" of individual grains, (3) "strain shadows" revealed in individual grains under crossed nicols, (4) bending or crumpling of individual grains. The first of these phenomena, *i.e.*, the production of cracks, indicates that the internal strength of the individual grains or the strength of their bonding to one another was exceeded by the stresses attending the folding, whereas the last three indicate that the folding stresses did not completely overcome the elastic strength of the material but produced, and were relieved by, internal deformation within the mineral grains.

The problem of determining relative ages of folding and of ore deposition might appear to be solved one way or the other dependent on whether careful microscopical examination detects or fails to detect evidences of deformation or strain as listed above. We believe the matter is by no means so simple; but it may be discussed effectively from the standpoint of those two contrasting cases.

THE CASE WHERE STRAIN IS DETECTED.

Where one or more of the evidences of strain just listed is found in specimens of ore from a folded structure, it is customary to conclude that the folding was post-ore. In many instances, such conclusion is probably sound. But in order that it may be undoubtedly valid, certain precautions are necessary which, so far as known, are not usually taken in the investigation of this question. For instance, original cracks and voids may be and in some instances are inherited by replacement as faithfully as the exact shape of an original crystal may be adopted by its pseudomorph. Striking examples of such pseudomorphous preservation of openings may be seen in (*a*) the retention of the cleavage cracks in a thoroughly kaolinized crystal of orthoclase, (*b*) the entire emptiness and perfect preservation of the original gas-cavities in a vesicular lava now profoundly altered from its initial composition, or (*c*) the complete alteration of crystals in a vug without any filling of the vug. In just the same way, irregular fractures of the usual sort may persist in the replacing material even though actually formed previously in the material which has suffered replacement. In short, the absence of mineral deposition in cracks or other voids within a mineralized mass is merely presumptive evidence that these openings were formed since the mineralization of the mass which contains them; it is not positive proof—these openings *may* have existed previously and have been preserved, unfilled, just as any other structural feature of the original rock may have been preserved. This may apply on any scale, large or small, and thus must be taken into account not only in the microscopical study but in the field examination as well.

In the second place, it must be remembered that rocks and ores, in the course of their existence, are subject to many other stresses than those due to a particular generation of folding. For example, stress sufficient to cause strain may result from temperature changes; from any chemical alteration which is attended by increase of volume and possibly from that which causes decrease of volume; from nearby igneous intrusion; from direct loading or

unloading higher up, as by erosion or by sedimentation or effusion; from isostatic adjustment (possibly?); or from regional crustal deformation of any one of many causes. Moreover, certain features that look like strain fractures may actually be such only in subordinate degree, if at all. It is common to find, for example, that the hard opaque constituents of an ore, like pyrite or magnetite, show "fracturing" which is absent in the surrounding softer material. The suggestion that the pyrite retains the cracks that were produced in it but that the surrounding minerals have flowed or recrystallized [6] has been shown not to hold in all cases, for similar "fracturing" is common in ores in undisturbed sediments,[7] therefore, the "fracturing" in these, and probably in most, cases took place while the ore was in process of crystallization and not as a consequence of later external deformation. Tolman and others have shown, moreover, that many of the "cracks" in pyrite are in reality places of corrosion-replacement by slightly later minerals of the same ore-period. Still more recently, Wandke [8] demonstrated that apparent shattering of pyrite along two systematic directions was really due to localized replacement along weakness directions (incipient cleavage) and that mechanical disturbance of the pyrite had been slight or wanting.

The finding of stress-effects or strains of one or more descriptions in a specimen taken from a fold is therefore no certain proof that these strains are the result of that particular folding. One or both of two steps must be taken in order to establish such proof reliably: (1) the nature, intensity, distribution and attitude of the strain or strains at any place must conform or harmonize qualitatively and quantitatively with the folding at that place, or (2) the evidences of strain must be shown to exist from place to place in proportion to the local intensity of the stresses which produced or accompanied the folding. That is to say, there must be local correspondence or areal correspondence between

[6] Emmons, W. H., ECON. GEOL., vol. 4, pp. 760, 779, 780, 1909.
[7] Graton, L. C., and Murdoch, J., *T. A. I. M. E.*, vol. 45, pp. 37–38, 1914.
[8] ECON. GEOL., vol. 20, pp. 605–7, 1925.

observed strains and the folding, or there must be both. These two requirements may be briefly discussed in order.

Local Correspondence.—Cracks arranged radially with respect to the center of the fold, when not accompanied by other cracks in haphazard directions, or when dominating these other cracks in number, width or persistence, may probably with safety be regarded as due to the folding. If such radial cracks are widest where farthest away from the center, *i.e.*, where tensional stresses are greatest, and become fainter or pinch out completely toward the center, where tension gives way to compression, their genetic dependence on the folding is further confirmed.[9] Cracks or shear planes disposed nearly or quite parallel to the bedding or other foliated structure, and thus coinciding approximately with the arc of folding at any given place, may be a consequence of the folding deformation and specifically due to the tendency toward slippage along the beds or foliæ when layers originally of uniform length are respectively stretched or compressed, depending on whether they lie on the outside or on the centerward side of the layer or curved plane called the "neutral surface" or "surface of no slip." Cracks parallel to bedding or foliation, however, may well have been present in the rock before it was folded.

[9] Obviously, radial cracks cannot continue indefinitely to increase in width from the center outward; where such radial fractures are developed, the actual situation as seen on a field scale is this: In a given folded layer or bed (or group of layers or beds which may, in this respect, act as a unit), any single radial crack is narrowest toward the concave (centerward) side and widest on the convex side of the bed; eventually, the stresses apparently find more effective relief by slippage along the bedding planes than by indefinite centrifugal continuation of the radial crack—in short, the widening radial crack comes to an abrupt termination at a bedding-fault. The bed or beds on the far side of that fault then become themselves traversed by new radial cracks which as a rule are out of line with those formed in the beds on the near side. And so the propagation of this combination of radial cracks and bedding-faults continues outward until the stresses become dissipated through so great a "fan" of rock that they are eventually absorbed without the production of fracturing.

It would seem that in certain instances entirely analogous results might tend to be formed on a microscopic scale, whereby the aggregate of mineral grains behaves somewhat like the aggregate of beds, but so far as known such features have not yet been recognized and reported. In the majority of cases, yielding probably occurs at the grain boundaries rather than as clean-cut plane cracks traversing directly across grain after grain.

Therefore the mere presence of curved cracks of this attitude is not proof of cracking due to the folding. To whatever extent these cracks give indication of shear or slippage along their two walls, the likelihood that they are due to the folding is probably increased; for if they did not exist until the folding began, each represents a place in and near which the deforming stresses accumulated and produced increasing strain until finally complete rupture took place, whereas if the fractures had been present before the folding, slippage along each would have been relatively easy and therefore no considerable internal deformation could result in the immediate walls.

Even if the cracks can be tied up genetically with the folding, it is necessary to prove that they have actually *formed in the ore* instead of having been inherited by it as already explained. If radial or concentric fractures carry in or along them minerals characteristic of the ore in the folded structure, there can be little doubt that the folding preceded ore deposition; but, as already pointed out, the finding of such fractures empty is not positive proof that folding followed ore deposition. Moreover, it must not be forgotten that fractures existing prior to ore deposition and apparently "healed" in the course of the subsequent mineralization may nevertheless remain as places of relative weakness so that later stresses of whatever nature or cause, and perhaps of relatively low intensity, might find relief along these earlier fractures and cause them to reopen and thus give a counterfeit appearance of *post-ore* fracturing on an important scale.

Pressure twinning and strain shadows in minerals of the ore period can probably only rarely at best be shown to be related, in attitude and placing, to the stress directions involved in the folding. In part, the difficulty is unavoidable—the necessary facts are not known. In part, the deficiency arises through the observer's lack of knowledge or use of facts that are known. In any case, the observer is ordinarily content to report "pressure twinning" or "strain shadows" without trying to determine, or apparently realizing that it is desirable to determine, anything more. For example, what effort is made to ascertain, in an ag-

gregate of differently oriented calcite grains, (*a*) which orientation of grain is most favorable for the development of pressure twins[10] by stress from a given direction, (*b*) whether pressure twinning is uniform in all the grains or on the other hand is so confined to certain orientations as to suggest the direction from which major stress was applied, (*c*) how much stress is required to develop pressure twinning and whether this magnitude of stress probably corresponds with the stress-intensity required to produce the observed folding? Analogous questions may be asked with regard to the production of strain shadows, which are in an even less satisfactory state of understanding.[11] Moreover, while twinning, when once produced, probably represents a permanent deformation, strain shadows result chiefly from elastic deformation which is likely to subside mainly or entirely when the causative stress ceases; therefore, if folding was produced and completed in, say, Mesozoic time, will strain shadows that may have been caused by that folding remain during the millions of years that have intervened? How certain are we that some of our strain shadows are not of such recent and artificial origin as the grinding of the thin section (or on the other hand that the preparation of the thin section may not have relieved stresses previously attended by strain shadows and thus destroyed the latter)? It is possible, however, that more careful observation of features like twinning and strain shadows than they have yet received would show that in certain instances they can contribute valuable testimony.

Generally similar comment to that just above may be made regarding the bending or breaking of individual grains.[12] It may be noted, moreover, that certain minerals of prismatic habit appear to be the victim of unknown stresses during growth even while none of the surroundings show any evidences of such

[10] Which usually develop along the base or the negative rhombohedron of half slope.

[11] Shadowy extinction due solely to radial or confused orientation at the time of crystal growth has unquestionably in some instances been mistaken for strain shadows and thus erroneously assumed to be due to later stress.

[12] Precautions that ought to be observed but are ordinarily ignored are given later.

stress; for instance, tourmaline crystals are found cracked into detached pieces and out of line even though in a matrix devoid of all signs of deformation.

This whole subject obviously requires abundant intensive study. In the meantime, conclusions should be drawn only with the utmost caution.

Areal Correspondence.—If evidences of strain of the kinds already noted are invariably found where there is folding, and if the strains are most evident and extreme where folding is sharpest; and conversely, if strains of similar nature are absent or rare where there are no folds, then the straining and the folding may be ascribed to the same cause and time. Ordinarily, however, the observer seems content to find strains in the folded ore and makes no attempt to prove (and perhaps none to ascertain) that they are not equally present in the unfolded places. Obviously such procedure is logically incomplete and therefore unreliable.

THE CASE WHERE STRAIN IS NOT DETECTED.

When, as is often the case, the field evidence is noncommittal, and when the microscope fails to reveal in the folded ore (or better stated, in the ore from folds) any of the evidences of strain previously enumerated, it is customary to conclude that the ore did not participate in the folding and therefore is later. Statements of such conclusion, evidently resting on no other evidence than the absence of strain, are common in the literature.[13]

This conclusion may be sound, or it may be false. Those who draw it, off-hand, evidently fail to take into account a variety of considerations, including certain relationships of scale imposed by magnification under the microscope, and thus they draw unreliable inferences. Various factors and conditions tend to limit the validity of microscopical interpretation of the kind here concerned. Among these may be noted:

[13] Since this article aims to be deliberative rather than controversial, specific citations to such conclusions are omitted. Those whom the shoe fits will doubtless prefer to put it on in private.

1. The nature and intensity of the folding.
2. The texture and mineral character of the ore.
3. The small area of thin or polished sections.
4. The small field of view under the microscope.
5. The inability by microscopical observation to detect bending of mineral grains except where the radius of curvature is very small, *i.e.*, where the folding is very sharp and tight.

These considerations may be discussed in order.

1. *Nature and Intensity of Folding.*—All that we know about rock folding indicates that the dominating stresses which produce and attend it are essentially compressive in nature. Folding of solidified rocks apparently goes on mainly, if not solely, at such depth that the rocks are under relatively great load. Although theoretically the outer or convex side of each folded member or layer is subject to tension (while the inner or concave side suffers compression) this tension is doubtless often only relative—that is to say, the compression on the convex side is less than that on the concave side.

Although there are exceptions, it is not common experience that limestone or shale or any other rock is weaker on a fold than in unfolded places; ordinarily it requires as much muscle to break off a specimen from the crest of a fold as from any other place. If folded rock is not fractured and weakened, why is it reasonable to expect weakness and fractures in folded ore? Shearing may be localized along the axial plane of a fold, but absence of shearing in many similar folds nearby proves that the shearing is later superimposed on certain folds instead of being an accompanying and inseparable result of all the folding.

In the main, folding is recognized by the presence of bent layers, not by the presence of fractures or other weakening conditions in the folded material. In many folded sediments, the folding is to be detected not in any feature seen in the rock itself but only in the bent shape of the bedding planes; if there were no bedding planes, the folding would be missed.

Bedded rocks, either sediments or lava flows, which have been notably folded are often found to rest directly on basal granite

or similar non-bedded rock along a contact which is tight, and which is bent parallel to the folds of the overlying beds. In such instances there can be no escape from the conclusion that the underlying granite or similar rock must have undergone substantially the same deformation as that revealed in the bedded rocks above, but examination of the granite by itself would never suggest the idea that it had been folded. Without the evidence afforded by the bedded rocks above, folding in the region would be completely missed. Presumably if the folds be sharp enough, deformation in granitic rocks might be detected. But so far as known to us, folding [14] in granitic rocks has never been established by direct evidence contained within these rocks. Of course where folding is tightest and most intense, the conditions of regional metamorphism have been such that the sub-sedimentary basement is generally strongly gneissic or schistose, and such rock, having a recognizable layering of its own, easily reveals any folding it may have undergone. The foregoing argument, therefore, as to the absence of recognizable folding in granitic rocks is not wholly compelling. But the fact remains that folding has not been actually detected in granular, structureless, rocks either in the large way in the field or under the microscope.

Detection of folding in the field apparently requires the presence of parallel lining such as bedding or foliation, which is extensive enough to disclose bending when these lines have actually been substantially bent. Detection of folding under the microscope apparently imposes similar requirements on a correspondingly small scale.

Conceivably if certain rocks were sufficiently brittle, or under inadequate load, or if the folding were accomplished with sufficient suddenness, fracturing of importance throughout the folded portions might attend the act of folding. But results indicating such conditions are not very commonly encountered; even such brittle rocks as quartzites usually *bend*, if they bend at all, instead of breaking. Folding appears to be that stage of dynamic meta-

[14] This excludes those structural features reported by Hans Cloos ("Tektonik und Magma," Berlin, 1922) which, according to our understanding, are supposed to have been induced at the time of intrusion and do not concern later deformation.

morphism next short of rock flowage. Indeed folding must accomplish such internal rearrangement of the rock grains as to constitute the beginnings of rock flowage; that this is true is attested by the frequent and marked thickening of the crests and thinning of the flanks of a folded member without any weakening of the material where either thickened or thinned. It is doubtless this incipient flowage, permitting rearrangement and rebonding of the constituent grains, which causes the average folded rock to be neither traversed by cracks nor friable and crumbly. On the assumption that folding is the forerunner of out-and-out flowage, the folded rock may be even tougher, stronger, better bonded than its unfolded equivalent.

We are again led to the conclusion that folding is difficult to detect except through the presence of *originally linear structures* [15] *of considerable extent,* so that deformation of these straight lines is readily noted.

There remains for consideration under this heading the question as to *how much* deformation or stress would be required to produce fracturing. Unfortunately, data on this subject are scanty indeed. Fracturing of a grain will not take place until the elastic limit of the grain has been exceeded. Certain of the elastic constants of individual crystalline grains of various minerals are known.[16] But, so far as we are aware, little or nothing is known as to how such grains would behave if surrounded on all sides by other grains. Likewise the strength of bonding of given grains to grains of like character is unknown; also the strength of bonding of unlike grains. Any experimental data that may exist on any of these points would be almost certain to require profound modification before they could represent the conditions that prevail at the depth where rock folding takes place. Therefore there is at present no way of knowing whether grains in a matrix of similar and dissimilar grains would react to deforming stress by fracturing, by slipping over one another, or by bending. As already emphasized, the mere finding of grain-boundary weak-

[15] Linear as seen in cross-section; actually planes.

[16] Smithsonian Physical Tables, 7th Revised Edition, 1920, pp. 102–103, 108; also Landolt and Börnstein, Tabellen, Aufl. 5, 1923, pp. 82–84.

nesses or of fractures across grains cannot be accepted as proof that the stresses which produced the folding were the cause of these. Neither can the failure to find such cross-cutting or bounding fractures, on the other hand, be taken as proof that the material has suffered no deformation. Deformation may have been expressed as bending, which, as later shown, has scant chance of being detected under the microscope; or it may have been expressed as grain-boundary movements or as fractures through the grains, both of which types of strain, as set forth hereafter, may escape detection even at high magnification.

2. *Texture and Mineralogy of the Ore.*—The average ore, like the average rock, is rather fine-grained. The average ore, even more than the average rock, consists of grains that are substantially equidimensional; the texture is dominantly granular. The grains of the several mineral varieties present are generally scattered haphazard through the mixture. The average ore, therefore, possesses none of that elongate habit best suited to reveal folding. Those uncommon instances in which the ore grains show a departure from equidimensional shape and in which elongated grains lie in parallel or sub-parallel arrangement are found almost invariably to be due to replacement of already-banded [17] or already-foliated rock, with pseudomorphous retention or inheritance of the banded or foliated structure by the ore minerals. Pre-ore development of foliation might well have been accompanied by folding or banded rock might be folded before replaced by ore; if so, the folding would be likewise inherited by the ore minerals. The detection of folded shapes in the ore minerals (if such *should* be detected) would therefore not necessarily prove that the folding was post-ore. Actual post-ore folding, superimposed on pre-ore foliation with or without folding, would constitute a problem certainly difficult of analysis and solution.

Some veins are notably banded (in the sense of crustified), and if such veins were tightly enough folded, the bent bands would doubtless disclose the fact; in other words, the bands or the band boundaries afford that type of "linear" structure sufficiently

[17] The word "banded" here is used in the sense of a banded shale or a banded sandstone, as distinguished from the idea of crustified as applied to mineral veins.

extensive to reveal folding. But banding of this kind is chiefly confined to ores of rather shallow-seated origin—too shallow-seated, as a rule, to be in the metamorphic depth zone where folding occurs. Therefore banding is of little help in the problem of detecting and interpreting folding.

The average ore consists of a variety of mineral constituents possessing differing physical properties. The earlier, tougher, harder minerals [18] are, as a rule, held in a matrix of softer, more yielding and not infrequently more or less sectile minerals; the older, harder minerals are also generally the more insoluble,[19] whereas the later, softer minerals, or at least some of them, like calcite especially, are subject to relatively easy rearrangement through solution and re-deposition on a highly local scale—that is, with insignificant transportation of chemical material. If such an ore were subjected to external deforming stress, it seems altogether probable that the softer, more soluble matrix minerals would permit such necessary rearrangement of the mineral grains and re-healing of strains by solution and immediate re-deposition that the resulting deformed ore would show little or no evidence of deformation unless this had been carried to a very advanced degree.

When stresses accumulate in an ore to such an extent that something must give way, the failure is much more likely to take place along the boundary between grains than by forming cracks right through the individual grains. Perhaps the chief reason why the boundaries should be weaker than the grain units themselves is that the individual crystal grain, by virtue of its systematic internal arrangement, reacts to all external deforming stresses in a unified way which tends to preserve the grain intact, but at the boundaries between these differently oriented grain units, the stresses are necessarily accumulated and concentrated, so here it is that immediate or eventual failure—fracturing—takes place. For a variety of reasons,[20] however, it is more dif-

[18] Gilbert, G., ECON. GEOL., vol. 19, pp. 668–669, 1924.

[19] Pöschl, V., quoted by Gilbert, *loc. cit.*

[20] In part arising from the nature and methods of preparation of microscopical sections, whether thin or polished.

ficult to *see* tiny fractures along grain-boundaries than equally narrow fractures that cut across otherwise single crystalline grains.[21] Therefore it follows that minute fracturing may frequently escape detection, even under the microscope. Proof of this may be seen in the presence of narrow, hair-like replacement veinlets (such as secondary chalcocite or covellite in primary sulphides) which plainly have been localized by minute fractures, but these fractures themselves are not visible and their presence would never have been suspected except for the development of these alteration products along them. Undoubtedly many such sub-microscopic fractures exist that have not suffered replacement along their walls, and therefore their presence remains unknown.

As will be pointed out later on, the amount of deforming stress accumulated within the small volume represented by the size of an individual grain probably rarely exceeds the elastic limit of such a grain; therefore, strains are not to be seen within such grains. Some ores consisting chiefly of a single hard mineral species like magnetite or pyrite may be poorly bonded and crumbly; other ores of the same grain size and the same composition may be as firm and tough as cast iron. It is possible that the crumbly condition is a consequence of deformation and that the massive condition exists because such deformation has not been suffered; but unless this can be shown definitely, it must not be taken for granted—the difference is much more likely to be due in the average case to the conditions of initial crystallization of the ore, or to conditions of subsequent incipient solution or alteration which almost invariably begins at the grain-boundaries.

3. *Small Area of Sections.* 4. *Smallness of Microscope Field.* 5. *Difficulty of Detecting Curvature.*—These three considerations are closely related and may conveniently be treated jointly. They all tend to limit the applicability and therefore the value of microscopical interpretation regarding structural features of larger scale.

[21] This is even more true along the boundaries between unlike minerals than along boundaries between different grains of the same mineral.

The average diameter of a thin or polished section for microscopical examination ranges from 1 to 2 cm. This is admittedly a short section of arc in which to detect any but the very tightest folding.

The field of view of the average microscope when using medium low-power objective, say 16 mm. focal length, covers about 2 mm. diameter of section. With increasing magnification, the field covered of course declines proportionately, so that with powers of say 1,000 diameters, a circle only about 0.1 mm. in diameter is available for study in any field of view. Obviously, the area of observation is very small in relation to the average folded structure as a whole. The diagnostic value of this small section of the ore structure, when viewed under the microscope, will plainly depend in considerable degree upon the care with which the sample is chosen to bring out most effectively the structural features that may be present or suspected. The chip for microscopical section must be taken from the most promising place on the fold. The most promising place in general will, of course, be the crest of the fold as compared with any place farther from the crest; but it will also be advisable to note whether the specimen is taken from the convex-outer or from the concave-inner part of the folded member, since these two parts are likely to show (or theoretically ought to show) contrasting features. In addition, care must be taken to insure that the plane of the thin or polished section is at right angles (or at least a high angle) to the axis of the fold instead of parallel to the axis—mere certainty that the section cuts across the bedding or foliation will not suffice, for it might do this and still be parallel to the axis of the fold and thus show no bending and possibly no fracturing; and finally, some means must be taken of surely identifying the convex and the concave sides of the fold. All this means that the microscopical section must be definitely oriented with respect to the fold and the orientation known at all times. To achieve this, it will probably be necessary in the average case to record definitely the orientation of the chip at the time it is broken off in the field, in such a way that this orientation may be recognized in the

microscopical section. Pretty surely this has but rarely been done in connection with the microscopical studies conducted to date.

Finally, it is necessary to consider the scale relationships involved in microscopical examination. Let it be assumed that we have a specimen of ore taken from the crest of a tight, sharp fold, prepared for microscopical examination with all of the precautions enumerated above. Let it be assumed, also, that the folding was subsequent to ore formation. The question then arises as to how much deformation of the mineral aggregate making up the ore may be expected; also, how little deformation can be detected; further, is the amount to be expected likely to be sufficient to be seen? Answers to these questions may be obtained either by trial or by computation. Computation may be considered first.

Lath-shaped mineral grains will be best suited to reveal deformation, so we may assume that these are present and are the subject of attention. It may also be assumed, in order to simplify the problem, that a given narrow, lath-shaped grain of a length equal to the diameter of the microscope field is so oriented with respect to the section and to the fold as to reveal the maximum of folding that went on, and further, that the curvature of this lath is ideally parallel to the arc of the folding through it—that is to say that this lath-shaped grain suffered the full deforming effect of the folding instead of having escaped part of the deformation by turning or slipping in its socket of surrounding grains.

Now let the radius of curvature of this lath be r and the length of arc to be considered, *i.e.*, the length of the lath, be s. The angle ϕ subtended by this arc is then s/r. If one end of this lath or arc be considered as fixed, the deflection of the other end from a straight line tangent to the lath at the fixed end may be designated Δx and its value is given by the relation:

$$\Delta x = r(1 - \cos\phi)$$
$$= r\left[1 - \left(1 - \frac{\phi^2}{2} + \frac{\phi^4}{4} - \cdots\right)\right].$$

spectively. Fig. 3, *D*, shows these arcs at the true scale ruled on the glass. It need hardly be pointed out that folds as tight and sharp as any of these three arcs would rarely be encountered in practise—surely folds tighter than the smallest arc would be uncommon indeed. For example, Fig. 1 shows a sharply folded structure in which the tightest folds (at the left edge) are about 0.5–1.0 cm. radius. In Fig. 2, a bent (and corroded) chlorite foil is shown magnified at 500 diameters; the actual radius of curvature in this case is about 0.25 mm.—an exceedingly sharp fold.

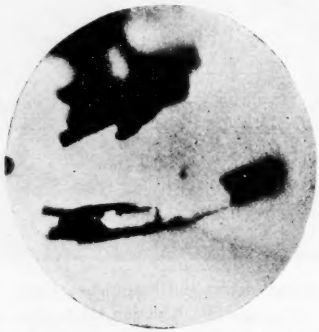

FIG. 2. Micrograph of curved and corroded chlorite foil in bornite, from Engels, California. (Reproduced from ECON. GEOL., vol. 12, Plate III., Fig. 12.) × 500. The foil has a length of about 0.08 mm. The actual radius of curvature necessary to give the gently curved arc here revealed is about 0.25 mm., or about the same as that at the eye end of an ordinary sewing needle.

Now when these arcs scratched on the glass slide are viewed under the microscope at the place where they are closest together, only a part of their length is seen, and of course, as the magnification is increased the length of arc in view decreases. Obviously therefore, the curvature of the arcs becomes less and less evident with increasing magnification. In Fig. 3, *A*, the curvature of all three [22] arcs is obvious enough, though apparently much "flat-

[22] It will be noted that the arc of smallest radius (greatest curvature) is a heavier scratch than the others; this permits identification of each arc in all the

tened" from the "true" curvature of Fig. 3, *D*. In Fig. 3, *B*, curvature is still evident, though much less noticeable. But in Fig. 3, *C*, curvature cannot be positively detected even in the smallest (right-hand) arc.

The conditions postulated in the immediately preceding discussion are all most favorable for the detection of curvature. In the average actual specimen these favorable conditions are not nearly approached. For example, curvature of a clean, sharp edge of a lath-shaped grain (or of a smooth circular scratch) reaching all the way across the field would be much easier to detect than if the edge of the grain were somewhat rough and ragged, as crystal faces, even, are often found to be when viewed under the microscope. Moreover, since the average mineral grain in an ore is not lath-shaped but chunky, and since good crystal faces [23] are the exception rather than the rule, detection of bending becomes all the more difficult.[24]

Plainly enough, then, the proper way to detect curvature is not to magnify but to demagnify. We have only to contrast the apparent curvature of the earth's surface with that shown by a small globe to realize the truth of this.

Finally, one serious lapse of logic appears to be chargeable against those who, failing to find in folded structures evidence of folding in the ore, conclude that the ore is of later introduction than the folding of the rock. Completeness of proof of this conclusion would plainly demand that *the expected microscopical evidences of folding must be sought for and found in the similarly folded rock adjacent to the ore,* since it is inescapable that the rock

photographs. A lizard-shaped scratch made by accident between the right-hand and middle arcs is to be detected in each of the three figures; this likewise serves to check not only the degree of magnification but also the identity of fields and of orientation in the three figures. The separation between the arcs in the three figures also measures the relative magnification.

[23] Except, perhaps, for the minerals of isometric form like pyrite, magnetite, cobaltite, etc., which are both physically resistant to deformation and of a compact shape making deformation difficult of detection.

[24] The most favorable conditions for detecting folding in an actual case would probably be found where bending of sharp cleavage planes of a mineral like mica is sufficient to be detected.

EXPLANATION OF FIG. 3.

A. Arcs ruled on glass slide. Radii of arcs, ½, 1, 1½ cm. × 66. Length of arc included in field, 1.33 mm. Curvature obvious.

B. Central portion of *A*. × 328. Length of arc included in field, 0.27 mm. Curvature evident only because lines are clean-cut and extend full width of field; in a rough mineral grain equally bent no curvature could be detected.

C. Central portion of *A*. × 1146. Length of arc included in field, 0.08 mm. Curvature not discernible.

D. Drawing to show true curvature of the arcs at natural scale. In order to avoid blurring or indistinctness in the printing of this figure, the drawing shows the lines of the arcs much heavier and notably farther apart than are the actual arcs on glass shown magnified in the other figures of this plate. The actual lines on the glass are about 0.002–0.003 mm. wide, or about 1/10 to 1/15 of the thickness of the average thin section; and the outermost arcs are actually about 0.058 mm. apart at their closest points.

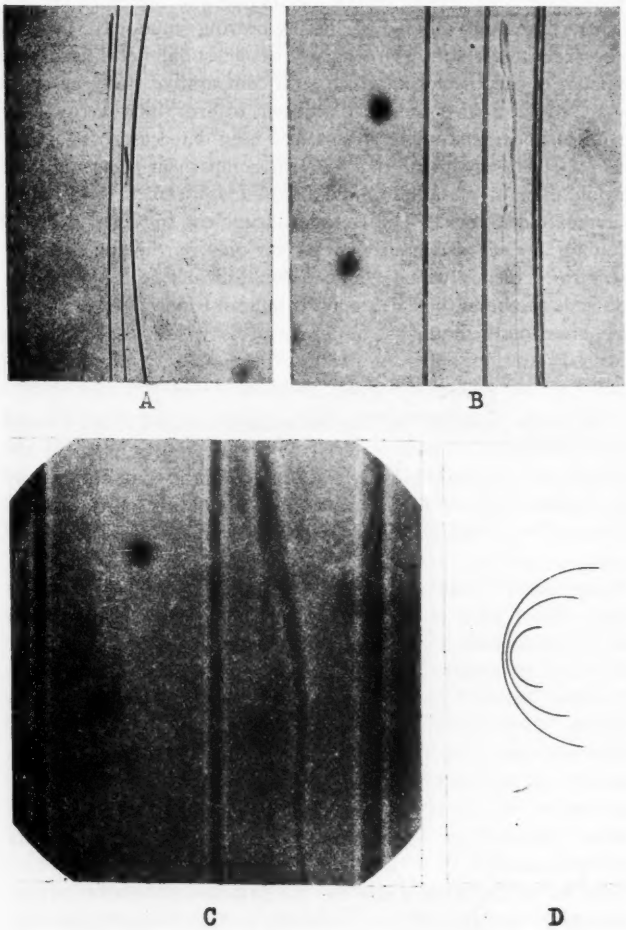

FIG. 3.

must have been present before the folding and therefore (except where thoroughly altered by the ore-bearing solutions) it should reveal whatever strains are capable of detection. No instance is known to the writers in which such comparative study has been made of the folded wall rock adjacent to ore which, because devoid of apparent folding strains, has been dated as post-folding. The chances are great that if such comparison were made, it would in many cases fail to find *microscopical* evidence of folding in the folded rock. The common argument in favor of post-folding age of ore therefore rests not only on the supposed absence of evidence, which evidence would in fact probably be very difficult to obtain, but also upon a logically fallacious or incomplete method of inquiry.

APPLICATION TO VEINS OF FILLING.

What has preceded has been deliberately confined to ore formed by replacement of rock material which previously occupied the precise position now held by the ore. Certain complications of presentation seemed thereby more easily avoided. It is evident, however, that bedding planes, foliation planes, fractures or other voids present in a rock may be folded when the rock is folded. Subsequent introduction of ore material as open-space filling in these folded voids or places of weakness would result in curved and sinuous veins and veinlets which would *look* as if folded, but which might actually be later than all the folding stresses. On the other hand, if these openings had been ore-filled before the folding, they would suffer folding along with the surrounding rock and thus yield masses similar in shape to those just mentioned but different in history and genetic significance. The problem of distinction between pre-ore and post-ore folding thus arises identically for deposits of open-space filling as for deposits of replacement.

So far as the writers can see, the principal arguments of the preceding section apply as forcefully and as completely to deposits of filling as to replacement deposits and the same conclusions must therefore be drawn for both.

APPLICATION TO GENERAL FOLDING.

The features and conditions that have been discussed in the foregoing pages obviously apply to *any* aggregate of mineral grains, whether of economic importance or not, whether metalliferous or earthy—that is to say, they apply in the main as well to rocks as to ores. Therefore it seems necessary to believe that the conclusions already reached apply to the microscopical detection of all folding.

CONCLUSION.

This paper seeks, primarily, to analyse the evidence by which folding of ore may be recognized in order that distinction may be made between ore which has suffered folding and, on the other hand, ore which has been introduced into rock structures that had already been folded. Among its chief conclusions are (1) that the evidences and criteria of strain are imperfectly understood; (2) that our knowledge of the elastic constants and other physical properties of minerals and mineral aggregates is woefully meagre, and (3) that the reasoning which has been applied to this question is for the most part loose and superficial and in part plainly unreliable.

Magnification, so useful and reliable in many ways, actually defeats rather than aids the detection of bending. The commonly-stated conclusion to the effect that the absence of visible deformation in ore studied under the microscope proves that the deformation was pre-ore is held to be logically deficient and as likely to be wrong as right. There are strong reasons for thinking that in many cases folding of importance which actually affected the ore would be classed by current methods of study as entirely pre-ore. Furthermore, it is submitted that the same difficulties in the way of detecting by microscopical means the folding of ore would apply to the detection of folding of rocks.

Obviously, then, the paper is essentially destructive in import. This the writers regret. It would have been far more gratifying and satisfying to have spent equal time and effort in pointing out new things that can be done, new tools that can be used, new ideas

which were independent of crystal banding. These were closely associated with sets of small parallel fractures of uncertain origin.

The cassiterite crystals themselves are highly variable in size even within a single specimen. With high magnification a crystal may cover a fair portion of the field, or it may be almost irresolvably minute, and all gradations may be noted between these extremes. The habit, while usually elongated transverse to the banding, is rather infrqeuently fibrous; more often distinct crystals may be distinguished. In a general way it may be said that the crystal size increases toward the periphery of a nodule. An unqualified increase was noted in nodules from Cornwall, England;[1] Durango, Mexico (two specimens); Catron, New Mexico; and Jefferson County, Montana. Specimens were observed displaying coarsely crystallized bands between the center and periphery of the nodules, from Durango, Mexico, and Montana (two specimens). Many nodules show no decisive change in crystal size throughout the interior (Durango, Mexico; Catron, New Mexico; Jefferson County, Montana, and from a locality unknown). The exterior of a nodule is frequently marked by the presence of coarse, usually non-elongated, pale cassiterite subhedrons.

Crystal bands are concentric with the general color banding trend. Nevertheless a single crystal may extend across several color bands, or several tiers of crystals may be involved in a single color band.

The association of specular hematite with cassiterite in wood tin is surprisingly common, indeed almost general, even in the most minutely crystallized nodules. The proportion of hematite present, however, is quite variable. In some nodules hematite predominates almost to the exclusion of cassiterite. In such cases the hematite occurs as aggregates of radiating blades or plates, individuals of which may attain a length of a centimeter or more. Between and upon these plates cassiterite may be recognized. Apparently, in this type of nodule, the wood tin has

[1] Unless otherwise specified, this feature was noted in only one specimen from each locality.

been largely deposited on a framework of hematite and it is therefore partly of later origin. Gradations can be found from this type to one in which the hematite is small in quantity and in crystal size. At this extreme, there are nodules in which the hematite does not occur in single radiating groups, but is distributed throughout the cassiterite bands as minute transparent flakes. It is interesting to observe that the hematite, in such cases, is confined to the inner portions of dark bands, and that both cassiterite and hematite crystals are of comparable order of size.

Replacement of Hematite by Cassiterite.—Cassiterite has replaced hematite. Partial and complete pseudomorphs of cassiterite after hematite blades and flakes appear in several thin sections. Good examples were found in the minutely crystallized nodules in which the original hematite had been confined to dark bands as the microscopic flakes previously mentioned. Here the pseudomorphs are outlined by dark cassiterite, with or without dusty residual matter. The cassiterite, moreover, occasionally is found as veinlets having non-parallel walls which transect the larger hematite flakes.

The replacement of hematite by cassiterite from Durango, Mexico, has been described by Genth [2] and also by Pirsson.[3]

An interesting structure may be observed in the Catron nodules: cassiterite forms sets of parallel projections along presumably rhombohedral directions of the hematite plates. This may be interpreted as cassiterite replacing hematite, or otherwise.

Discussion.—In those nodules which contain coarsely crystallized hematite blades, it is apparent that the banding does not completely envelop the blades. In thin section it may be seen that the blades interrupt the continuity of the cassiterite bands; the bands are considerably deflected by the blades but do not enclose them. Even in hand specimens it is noticeable that many blades project out beyond the wood tin (Durango, Socorro, Catron). This condition is quite different from that found by

[2] Genth, F. A., "On the Occurrence of Tin Ores in Mexico," *Proc. Am. Phil. Soc.*, vol. 24, p. 31, 1887.

[3] Pirsson, L. V., "Minerological Notes," *Am. Jour. Science*, vol. 42, 3d series, p. 407, 1891.

Knopf in the Lander County, Nevada, nuggets. Knopf observes:[4]

> The banding completely envelops the inclusions [hematite plates] and forms a series of concentric circles and ellipses around them. If the banding were due to "crustification," however, each band would be merely deflected by an inclusion and would envelop the inclusion only part way round, instead of completely surrounding it. This is believed to be a valid criterion by which banding due to the Liesegang phenomenon may be distinguished from banding due to crustification.

Thus, according to Knopf's criterion, the banding observed by the present writers must be regarded as crustification of some sort.

Furthermore, Knopf found that, in the Lander County nuggets, the bands displayed an increasing spacing from the nucleus outward, a feature in harmony with the Liesegang ring phenomenon. The writers examined eighteen thin sections with a view to determining whether this feature was general, with the following results:

	Band spacing decreasing from center outward	Band spacing increasing from center outward	Band spacing constant, not markedly increasing or decreasing, or otherwise indecisive
Total number of specimens	4	1 good example 2 doubtful examples	5
Localities	Durango Cornwall Montana Catron	Durango Durango (doubtful) Catron (doubtful)	Montana (4 specimens) Durango

The remaining six thin sections were so confused that no decision and tabulation were possible.

The results appear to show no *general* directional trend in change of band spacing.

Besides these criteria, there is evidence in several thin sections that a break in cassiterite deposition had occurred while colorless crystals of various species were deposited as crystalline crusts

[4] *Op. cit.*, p. 657.

between successive bands of wood tin. A particularly striking example of this was found in a nodule from the Bitterroot Range, Montana, in which acicular crystals of a transparent mineral have their bases accurately aligned on an inner wood tin band while their opposite ends project into various different levels of the overlying, undisturbed cassiterite band. The alignment of bases and non-alignment of opposite ends would be remarkable unless the transparent crystals had been deposited on a smooth surface and subsequently covered. Obviously the outer wood tin has merely molded itself about the previous crystals and is of later origin than them and the inner wood tin. Another example of crustification was found in a wood tin stalactite from Durango; between the cassiterite bands of this specimen are intercalated layers of concordant thin hematite lenses, the whole structure being concentric to the elongated axis of the stalactite.

Structures resembling contraction cracks were encountered in only one thin section (Durango). Apparently their general absence from other specimens means either that the wood tin examined was deposited originally in a crystalline condition, or that it was deposited in thin colloidal layers or films, each of which crystallized before any great amount of new material was added. There is reason to believe that a thin colloidal film, in the process of crystallization, may escape cracking while a large lump, less plastic because of its greater thickness, would be subject to it.

If the wood tin was deposited as successive crusts, what, then, is the origin of the color banding? There is a marked relation between the color of a band and its proximity to hematite. Usually the cassiterite is a deeper red where adjacent to hematite, and in specimens where the minute hematite flakes occur, these are confined to the base of dark bands. It is also noticeable that the cassiterite, as a whole, is more deeply colored toward the centers of nodules where hematite is more abundant. Furthermore, the pseudomorphs of cassiterite after hematite flakes are darker than the surrounding wood tin. Two explanations for this relationship may be advanced:

(*a*) The medium responsible for deposition first deposited hematite. Then, as it became supersaturated with respect to cassiterite, but still rich in iron, it deposited iron rich (deeply colored) cassiterite. As the medium gradually became depleted in iron the wood tin deposited changed to the lighter variety. Subsequent fluctuations in iron content caused corresponding changes in the color of the wood tin and occasionally brought about a precipitation of hematite. Specimens composed of bands which become lighter in color away from their hematite-bearing bases find very ready explanation in this way.

(*b*) The replacement of hematite by cassiterite entails the liberation or solution of iron, which would become immediately available for incorporation into the newly deposited wood tin. This would, of course, be the wood tin close to the earlier hematite.

It would seem that the process of diffusion would not adequately account for the disposition of color banding. The crustification banding and thus the final surface of nodules is most irregular, due, in part at least, to the disturbing effect of inclusions located at their centers and intercalated between the various depositional bands. It would, indeed, be remarkable if diffusion rings of coloring material should so exactly coincide with the crustification structure as the color bands have been observed to do. This coincidence of structures must be ascribed to the same cause—deposition.

In certain specimens the cassiterite is extremely fine in grain, even fibrous, and is intercalated as thin bands between layers of opaline silica. In such cases a colloidal origin for the cassiterite may be favored. But no evidence could be obtained to prove that all the nodules examined were colloidal in origin. In the specimens of coarse and variable grain, bands may be observed between the center and periphery which display the largest crystals, as previously noted. Furthermore the peripheral layers are frequently composed of coarse, pale cassiterite crystals with outer crystal faces. Such banding is perhaps most easily explained as original crystalline deposition.

Von Veimarn's law of corresponding states[5] enunciates the principle that the degree of dispersion, *i.e.*, the smallness of the crystal (colloidal or crystalloid), is directly proportional to the degree of supersaturation of the depositing solution. Thus no sharp boundary can be drawn between cassiterite deposited as a colloid or as a crystalloid. In hypogene solutions[6] one would expect that, as the solution cleared itself of tin during its ascent in the vein, by a perhaps rapid, possibly truly colloidal, deposition of cassiterite, thus becoming less supersaturated, the physical character of the cassiterite deposited would change, becoming coarser in grain. The final deposit, coming from weak solutions, would be the well crystallized cassiterite observed as the peripheral bands of the nodules. Coarsely crystallized intermediate bands followed by minutely crystallized ones would be interpreted as a renewed activity in the vein, the advent of new solution again high in tin.

The cause of the extreme supersaturation giving rise to hypogene colloids is a matter of speculation. In the realm of deposits genetically connected with igneous activity, colloids are not characteristic of deep-seated deposits but are usually confined to those of near surface origin. It seems reasonable to suppose that the rapid temperature gradient which must obtain near the surface of the earth's crust would favor a speedy chilling of solutions as they rise from depth, in consequence of which they are rendered supersaturated with respect to certain minerals before such dissolved material can diffuse to previously formed nuclei of crystallization.

Evidence for recrystallization was found in only one specimen (Durango). In the groundmass of dark, minutely crystallized cassiterite there were developed numerous larger elongated individuals of a light brown color, radially disposed, and traversing several minor color bandings. These (presumably recrystallized) "phenocrysts" had feathery edges and were immediately

[5] See Washburn, Edward W., "Principles of Physical Chemistry," pp. 432–440, New York, 1921.

[6] The writers do not wish to be understood as excluding the possibility that wood tin may also be deposited by supergene solutions.

bordered by a thin zone of dark dusty material, possibly hematite thrown out upon recrystallization.

SUMMARY.

1. Wood tin from several localities is described, emphasis being placed on certain points of resemblance and gradation between specimens of different occurrence.

2. Hematite is almost universally present.

3. Cassiterite replaces hematite, at least in several instances.

4. The color banding of the wood tin investigated cannot be ascribed to the Liesegang phenomenon.

5. Structures resembling contraction cracks were found in only one specimen.

6. The wood tin examined was formed by a crustification process and was deposited either as layers of crystals or as colloidal films subsequently recrystallized.

7. The color banding is a deposition phenomenon, probably due to variation in the iron content of the depositing solutions, or due to replacement of hematite.

8. The deposition of crystalline and colloidal cassiterite is discussed briefly.

9. The cause of hypogene colloidal deposition is referred to the rapid chilling of ascending solutions which the sharp temperature gradient near the earth's surface would favor.

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MINERAL ASSOCIATION AT THE GEORGE GOLD–COPPER MINE, STEWART, B. C.

W. V. SMITHERINGALE.

STEWART, B. C., is situated at the head of Portland Canal, a long narrow fiord which penetrates the Coast Range Mountains and forms the southern boundary between Alaska and British Columbia. The district is in the well-mineralized zone which extends along the eastern contact of the Coast Range Batholith from Alice Arm northwest through Anyox and Stewart. Locally it has been made prominent by the phenomenal richness of the Premier Mine.

GENERAL GEOLOGY OF THE STEWART DISTRICT.

The rocks of the district are composed of an older and younger series of argillites between which is a thick series of greenstones. The exact relations between these rocks are not known definitely, but there seems to be a transition from the greenstones to the argillites both above and below as shown by intercalations of tuffs and argillites.[1] These rocks, known locally as the Bitter Creek argillites, Bear River greenstones, and Nass River argillites, were folded during late Jurassic or early Cretaceous time and invaded by the Coast Range intrusives. The plutonic rocks vary from gabbros to granites, and give definite evidence of progressive intrusions; there is a suggestion that the later intrusions became more acidic. Mineralization occurred at some intermediate stage, as deposits are found in diorite or granodiorite rocks but granite dikes cut the orebodies in places.

GENERAL SYNOPSIS OF ORE DEPOSITS.

The mineralization occurs as quartz veins in some of the intrusives and argillites. The most important deposits, however,

[1] McConnell, R. G., Canada Geol. Survey, Mem. 32, pp. 13–18, 1913.

so far discovered are confined to the Bear River greenstones and occur in these as replacements along fracture zones.

The ore deposits may be roughly divided into the galena-sphalerite type and the chalcopyrite type. The first is characterized by pyrite, sphalerite, galena, tetrahedrite and the silver sulpho-salts. Gold and silver may be the metals of prime economic importance or may add important accessory values to the base metal content. Chalcopyrite is present in subordinate and varying amount. Secondary enrichment may be present in these deposits to varying depths depending upon the local fracturing and vein texture. The chalcopyrite type has a simple mineralogical association consisting of chalcopyrite, and pyrite with varying amounts of arsenopyrite and hematite and, in places, pyrrhotite; gold and silver are accessory with the gold predominating. No secondary enrichment of economic importance has occurred.

THE GEORGE GOLD-COPPER MINE.

The George Gold-Copper Mine is located on the south side of Bear River about seventeen and one-half miles from Stewart. The elevation of the valley is 1,100 feet and the main showings are between 4,300 feet and 4,800 feet.

Geology.—The country rock at this locality is the Bear River greenstones. The base of the mountain is mostly massive volcanic rock; at 3,000 feet elevation there is a prominent zone of calcareous and argillaceous tuff, some 60 feet in width, extending across the hill. Above this zone the clastic nature of the volcanic rocks becomes more marked and from elevation 5,500 feet to the summit (7,000 feet plus) the bedding is distinct. The whole series at this place is folded into a shallow syncline striking and pitching northwest. The western limb steepens abruptly on the western portion of the property but eastward, shallow dips predominate. Fracturing and faulting is evident but the extent of the deformation was not determined. The two main sets of joints appeared to strike northwest and northeast, dipping southwest and southeast, respectively.

Mineralization.—The mineralization of most importance has occurred as replacement along two main fractures (Fig. 1). While the wall rocks are considerably altered and impregnated with pyrite, the minerals of the vein proper are confined within a relatively distinct zone varying in width from 4 to 15 feet.

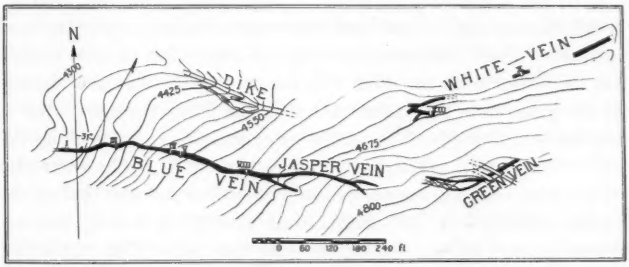

FIG. 1. George Gold-Copper Mining Company.

The vein striking northwest has been controlled by a zone of more intense fracturing than the veins striking northeast. The walls of the former are generally marked by 6 inches to 1 foot of broken wall rock rather well impregnated with pyrite and small amounts of arsenopyrite and chalcopyrite. In places where excessive surface leaching has occurred, faint ghosts of the original fault breccia may be seen. The northeast veins, especially the White Vein, though controlled by sharply defined fissures, show the processes of replacement much more clearly. Progressive replacement of the containing fragmental volcanics is well illustrated.

The mineral association is simple and shows pyrite, arsenopyrite, quartz, hematite and magnetite, and chalcopyrite. The paragenesis as shown by polished sections is constant throughout with minor variances in pyrite, quartz, and arsenopyrite. The deposit is of interest because of the unique relation of the oxide minerals, hematite and magnetite, to the sulphide minerals, and also for the pronounced concentric banding shown in different

places along the veins. This paper will describe these points and attempt to give an explanation of them.

Vein Structure.—The banding in the veins is best shown in the Jasper and Green veins and Cut XIII. White vein. These places exhibit a pronounced banding parallel to the strike of the vein due to the alternating layers of quartz, jasper, chlorite, and altered country rock; local and minor convolutions are many and intricate. Such characteristics suggest deposition in open spaces, but the writer does not think that this is possible. In the vicinity of the junction of the Jasper vein with the Blue vein the former is banded over a width of 15 feet or more and this extends along the vein for 200 feet, averaging 5 to 6 feet in width. The extension of the Blue vein has a southwest dip of 65° to 70° and that of the Jasper vein 45° to 50° S.W., thus producing a long narrow wedge-shaped mass of rock between the two. The northwest portion of the Green vein shows a block some 150 feet in length and 15 to 20 feet wide bounded by banded vein material. In Cut XIII. White vein the banding is very marked, but 10 feet to the southeast of this cut the quartz vein with its crustification has apparently disappeared; the surface rocks give no indication of its existence. The writer does not think that these conditions favor the existence of open spaces.

Further indirect evidence supporting this contention is obtained by considering the uniformity of peak elevation throughout the district. This suggests a peneplain, the remnants of which show a gradual rise in elevation from 6,000 feet in the south about Stewart to 8,500 feet in the northern and eastern section of the mountain range, in which section this deposit occurs. These elevations have most likely suffered considerable modification due to Tertiary, and later Pleistocene glacial, erosion. The latter has been severe in scouring and rounding many of the higher mountains, and producing deep gorge-like valleys, in some instances several thousand feet deep.

Considering these facts and the elevation of the vein outcrops (4,500 feet) it is seen that the deposit was formed at least 4,000 feet below the surface and probably at greater depths, for the

mineralization was produced in the upper Jurassic [2] following the uplift of the Coast Range and the intrusion of the Coast Range Batholith, and peneplanation of the range occurred during the Cretaceous.[3] This erosion was sufficiently deep to unroof and remove the upper part of the batholith. Re-elevation took place during the early Tertiary.

From this viewpoint it is seen that besides the 4,000 feet mentioned above, there was also removed a great thickness of overlying material which formed the roof of the batholith. For these reasons it is thought that the deposit was formed at depths approximating 8,000 feet or more. It seems likely, therefore, that some mode of formation other than deposition in open spaces was responsible for the banding.

It is suggested that the strain produced during folding and uplift was released principally along main joints in which some fault breccia was produced, but there were also minor adjustments along parallel slip joints for a distance of several feet on either side of the main fractures. Such a phenomenon would give the blocks between the fissures ample support and likewise produce conditions favorable for the circulation of solutions. The banding is the result of solutions ascending along these slip joints and replacing the rock outward from them. The minor convolutions are the result of local replacements about fragments in the original volcanic breccia. The contact between such fragments and the enclosing matrix would be a zone of relative weakness along which the intruding solutions would work. This concentric banding and progressive replacement of the fragments of the breccia is well shown in Cut X. White vein by intricate colloform banding of chalcedony about fragments which clearly show the gradual transition from the central partially altered nucleus outward. The outline of the fragments is retained in ghost-like form, emphasized by the colloform banding.

Paragenesis.—The paragenesis of the minerals as shown by polished sections is generally older gangue, hematite, pyrite,

[2] S. J. Schofield, Canada Geol. Survey, Mem. 132, p. 42, 1922.

[3] *Idem*, p. 31.

arsenopyrite, quartz, hematite and magnetite, chalcopyrite, bournonite (?), barite, and covellite. There are local variances in the order of pyrite and arsenopyrite, but as evidence supports the existence of a continuous deposition of pyrite with decreasing pulsation in the intensity of precipitation, such variances are to be ex-

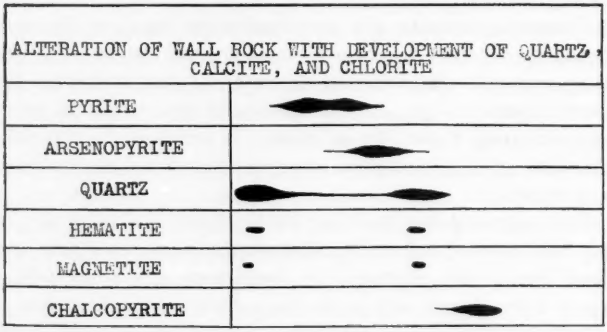

FIG. 2. Graphical illustration of paragenesis.

pected. Figure 2 attempts to give a graphical presentation of the different phases during deposition of the principal hypogene minerals.

Pyrite.—Pyrite is one of the most plentiful sulphide minerals present. It developed early, in two stages, as shown by a yellowish-white variety being replaced by a later more yellow pyrite. The interesting features of the pyrite are its occurrences in plate-like forms and in crystalline aggregates arranged in circular form about centres of quartz or chlorite. The plate-like forms are made up of an aggregate of grains or in the smaller examples of one grain of pronounced prismatic habit. Such a phenomenon might result from a shearing of a pyrite mass, the resultant granular aggregate being arranged in prismatic habit bounded by shear planes. The other alternative is pseudomorphism after some prismatic mineral. As branching forms are frequent, the latter seems to be the more reasonable. The only mineral present

which occurs in similar crystal outline is hematite, and such occurrences of pyrite are assumed to be pseudomorphic after hematite.

The circular masses of pyrite about cores of quartz or chlorite suggest a primary colloidal deposition with subsequent crystallization. These forms are common, though not plentiful, throughout the sections studied. A particularly interesting polished section was obtained from the tunnel on the lower zone, some 1,000 feet below Cut I., which gives considerable information as to the colloidal origin of these forms. This specimen is taken from the zone of calcareous tuffs previously mentioned, which in this area contains exceedingly fine-grained pyrite which is either disseminated through the rock or confined to small veinlets. There is also a little chalcopyrite. Hematite is found in small stringers up to 3 or 4 inches in width cutting the sulphide minerals. The specimen was taken from a small veinlet, of very fine-grained pyrite, cutting the tuffs. Fracturing has occurred in the veinlet and in these fractures jasper has been formed. Colloform banding of pyrite is clearly seen with the unaided eye. Microscopic examination confirms this structure and shows a radial jointing extending through the several concentric bands of the concretionary forms. It is also observed that the fine disseminated pyrite exists as minute spherules, either solid or as a ring about a centre of gangue. Magnetite and hematite are also present, both developing in small crystals or as small spherules exactly similar to the pyrite. In some instances one-half of a ring may be made up of pyrite and the other of one of the oxides. These relations tend to show that the pyrite was deposited in a colloidal state and at high temperatures. Most of the gangue is calcite, and the writer believes that the CO_2 present has had a direct effect on the formation of the iron oxides.

Arsenopyrite.—This mineral is present in varying amounts in all sections studied and varies in its relative concentration in the vein from place to place. While common throughout, it shows a considerable concentration in the Blue vein about half way between Cuts III. and IV. and again just above Cut VIII. In the

main, this mineral developed later than pyrite; it occurs partly in masses and scattered grains of rhombic form but a considerable amount is present as allotriomorphic grains; a few scattered bunches were observed that duplicate the prismatic forms mentioned under pyrite. The arsenopyrite veins and replaces the pyrite. In the latter rôle minute cores of pyrite may often be seen and in some instances the pyrite assumes a white cast and the pyrite-arsenopyrite contact looses its distinction; the extreme is marked by a faint yellowish blotch in the center of the replacing arsenopyrite. Such a replacement is taken to represent a working over of original pyrite by solutions rich in arsenic with a resultant substitution of arsenic for sulphur. The allotriomorphic forms of arsenopyrite are taken as pseudomorphs after pyrite. It was interesting to note the development of arsenopyrite in a series of crystals on the pyrite, particularly the lath-shaped forms. The surfaces of the pyrite grains produced an increased concentration of the solutions due to adsorption resulting in the precipitation of arsenopyrite and the growth of these crystals perpendicular to the adsorbing surface.

Quartz.—From the combined study of thin sections and polished sections the quartz deposition may be divided into two main stages; the first comprising the alteration of the enclosing rock prior to sulphide precipitation; and the second occurring in lesser quantity after the main formation of arsenopyrite but before chalcopyrite. Deposition of quartz continued during the intervening interval.

The initial development is best shown in those sections of the veins exhibiting parallel banding. The megascopic description and probable origin of this banding have already been given. The specimen collected as typical of this banding is of interest in giving the possible mode of deposition of the silica. The hand specimen is about 5 centimeters across and shows, from one side towards the center, altered volcanics which now consist of chlorite, a greenish calcite, and varying amounts of quartz. Inside this is a layer of concentrically banded quartz, about 3 mm. wide, a film of jasper, another concentrically banded zone of quartz 2 mm.

wide, a second film of jasper, and in the center a zone of quartz 1½ to 2 cm. wide. The remainder of the specimen is a duplication of jasper, quartz, jasper, quartz and altered wall rock. The center zone is well crystallized with the crystals occurring in radiating groups; along the inner jasper band they are arranged more or less perpendicular to the walls and impart to the center veinlet a crustified appearance. Individual crystals 3 by 1 mm. are common.

The study of a thin section across the narrow zones of concentric quartz and jasper and extending well into the central band is of particular interest. Each zone of the laminated quartz is seen to be made up of layers of coarse quartz separated by thin films of a very fine-grained quartz approaching the chalcedonic stage. The coarse quartz is crystallized in individuals which extend approximately perpendicular to the banding, and which have a peculiar wavy extinction. The jasper films are a mixture of chalcedonic quartz, calcite, and specularite. The quartz in the main center zone is a duplication, on a larger scale, of the coarse quartz in the thin bands. The grains are intergrown in a very irregular manner, nearly all showing hypidiomorphic outlines, with the prism well-developed. The undulatory extinction is striking. Numerous large crystals clearly show their development from an aggregate of minute grains, the original boundaries of which still remain in ghost-like form within the larger one. By reflected light only, very distinct and delicate colloform banding may be observed. The same section with transmitted light and cross nicols shows the crystallization of the quartz more or less perpendicular to the banding and conforming with it. These characteristics are common, in varying degree, to all the thin sections studied.

From such evidence as this it is believed that a large portion of the quartz present in the veins was deposited in the colloidal state and later crystal growth took place. During the solidification and subsequent dehydration and crystallization, shrinkage cracks were developed and these were healed by the chalcedonic quartz. If one accepts the criteria that are commonly advanced as evi-

dence for colloidal origin, then it must be granted that a large portion of quartz found in these veins, as represented by the specimens and thin sections studied, is derived from colloidal solution.

In the chalcedonic quartz and in veinlets and scattered bunches through the recrystallized quartz is a carbonate (mostly calcite with some siderite), and in this carbonate hematite is developed. The size of the crystals varies from minute semitranslucent forms to those easily discernible megascopically. That the formation of hematite was controlled by the carbonate is evident. A vein chlorite high in iron, is also intergrown with the carbonate. This chlorite is formed along cracks in the quartz and was deposited at a later stage than the quartz and carbonate.

After the formation of pyrite and arsenopyrite, the deposition of quartz veined and shattered these minerals and scattered the fragments about to some extent; there has been some replacement of the sulphides by the quartz. Hematite appears to have crystallized, in fine lath-shaped forms. Its exact relation is ambiguous but the presence of associated jasper in which incipient hematite crystals are developing suggests a sol containing silica and ferric hydroxide, which being oppositely-charged dispersed phases, neutralized one another electrically and deposited simultaneously as a gelatinous precipitate or gel which then crystallized as specularite and quartz. (The neutralizing effect would persist only so long as any positively charged ferric hydroxide remained.) Such a process is suggested to account for the apparent contemporaneous crystallization of specularite in quartz and is not to be interpreted as the cause of the main deposition of colloidal silica. In other places the jasper and specularite are seen to be developing along with a replacement in the older quartz. Both these occurrences in polished sections are borne out in thin section. The latter mode of jasper, *i.e.*, finely disseminated specularite along with a replacement in quartz, is seen in thin section to be a mixture of chalcedonic quartz, specularite needles, and a carbonate.

Hematite and Magnetite.—The occurrence of hematite has been discussed in general under Pyrite and Quartz. There it was observed that the hematite was deposited in two stages; the first as one of the initial alteration products of the country rock, and later, after arsenopyrite but preceding chalcopyrite. The occurrence of hematite is of interest not only because of its deposition in a stage intermediate in the sulphide sequence but also due to its later alteration to magnetite which faithfully preserves the specularite crystal form in a great many instances. The deposition of iron oxide after sulphides has been noted at the Cash mine, Hassayampa District, Arizona, by Lindgren,[4] who states that:

> . . . the place of the iron oxides in the succession being between pyrite and chalcopyrite. . . . It appears that the deposition began with magnetite, quartz, and calcite, after which pyrite with quartz and calcite was deposited in small crystals and crystal aggregates. Then followed specularite in long laths in quartz; parts of the laths and masses of specularite consist of magnetite, suggesting all of the specularite had passed through an earlier magnetite stage.

It is to be noted that the relative sequence of mineral deposition has been very similar. There is, however, in the deposition at the George Gold-Copper Mine, the distinct difference that hematite has been formed first and later altered to magnetite. The time of this alteration is not shown in any of the sections, but it is assumed to have taken place prior to the deposition of the chalcopyrite.

The cause of this occurrence at the Cash Mine is assigned to the rise in the temperature of deposition due to the intrusion of many dikes into the surrounding rocks. Running parallel to the Blue vein (George Gold-Copper Mine) is a dike, as shown on the map, but it does not seem possible that this has produced any effect on the vein minerals. It was intruded after the complete formation of the vein and as such could have had no effect on the deposition of the hematite. Its effect on the change of hema-

[4] Lindgren, W., "Ore Deposits of the Jerome and Bradshaw Mts. Quadrangles," U. S. Geol. Survey, Bull. 782, p. 122, 1926.

tite to magnetite is very doubtful because if such was the case one would expect to find a change in the sulphides as well, but none is apparent. It has been previously stated that the specularite was deposited in association with calcite, and it is contended that during this brief phase of mineralization the local conditions existing in the vein favored the oxidization of the iron by CO_2 and resulted in specularite being deposited. The readjustments just prior to the chalcopyrite deposition resulted in the conditions changing from oxidizing to reducing ones, and it was during this change that the subsequent conversion of specularite to magnetite, with its content of ferrous iron, took place. Other specific examples of the alteration of specularite to magnetite have been noted by W. H. Callahan[5] at Cornwall, Pennsylvania, and in a suite of specimens from Terre Neuve, Haiti. At Cornwall, Pennsylvania, there is an iron ore deposit of contact metamorphic origin in association with a diabase sill cutting a series of limestones and limy shales. The minerals present are specularite, magnetite, chalcopyrite and pyrite, associated with biotite, tremolite, and pyroxenes. These minerals are almost entirely confined to the shaly member. A microscopic study of these ores showed that the specularite was the early iron oxide developed, and this was later altered to magnetite which retained the crystal form of its host. This phenomenon occurs at the outer contact of the deposit against the limestone in contrast to the normal isometric magnetite characteristic of the remainder of the orebody. The magnetite replacement of the specularite is most pronounced in the deepest portion of the mine. The close association of the ferric oxide to the limestone contact again suggests the influence of released CO_2 in oxidizing the ferrous solutions and precipitating specularite.

In a recent paper by Butler[6] it is claimed that both CO_2 and H_2O may act as oxidizing agents in hypogene solutions. The

[5] The magnetite deposit at Cornwall, Pennsylvania. Thesis submitted for partial requirement for degree of Master of Science, Mass. Inst. Technology, Dept. Geology, 1927.

[6] B. S. Butler, "Relations of Oxygen and Sulphur Minerals in Ore Deposits," Econ. Geol., vol. 22, pp. 233–245, 1927.

maximum effect of the CO_2 being at 500° C. and of the H_2O at 600° C.; the oxidizing effect of both decreasing at both higher and lower temperatures. SO_2 acting as a reducing agent has its maximum effect at 350° C.; above this temperature its action is reduced.

Chalcopyrite.—Chalcopyrite is the remaining sulphide of common occurrence and is the mineral of main economic importance. It developed after the second phase of quartz deposition but very closely associated with it, because in many instances the relations of the quartz and chalcopyrite are so intimate that a contemporaneous deposition is strongly suggested. The chalcopyrite is frequently intimately intergrown with pyrite and arsenopyrite and shows many minute veining and replacement patterns with these minerals and all stages of replacement of both.

Gold is present in small amount and a study of the assays shows that the gold is associated with the arsenopyrite since the gold value varies directly as the content of this mineral in the vein. No gold could be distinguished under the microscope.

Microscopic specks of a soft semi-brittle white mineral were observed scattered through the chalcopyrite. These were too minute to carry out any definite tests but such reactions as were obtained, combined with its physical properties, suggest a sulphosalt such as bournonite. If this is so, the small but rather persistent silver values obtained in the deposit are assignable to this mineral.

Barite is developed in the center of the vein at Cut V. in considerable quantities and elsewhere in much smaller amount. It also occurs in veins up to 6 inches in width cutting the volcanic rocks in various places on the property, and has associated with it small portions of chalcopyrite and hematite. In one or two polished sections barite was sparingly present, replacing quartz and chalcopyrite. In the latter instance minute specks of covellite were observed in the barite. This phase is taken to represent the closing stages of hypogene deposition.

The weathering changes are slight and are confined to fissures The supergene minerals are covellite and limonite. The former

is confined to minute cracks ramifying through the chalcopyrite. Limonite is developed as a brown and a blue variety, the latter occurring in rhombic outline and to some extent locally in minute veinlets. The possibility of this variety being pseudomorphic after arsenopyrite is a debatable question. The brown variety is an alteration of chalcopyrite, magnetite, and hematite. This mineral is not different from magnetite in color or relief but is distinguished by its streak, and etching with $SnCl_2$ dissolves the limonite and leaves the magnetite with a brown etched surface. This reaction sometimes shows an intimate replacement of magnetite by limonite and the relations indicate that the magnetite is being oxidized along with the other minerals.

Origin and Nature of Solutions.—The nearest outcrop of a plutonic rock is about one mile northeast of the main veins shown in Fig. 1, and about 2,500 feet below their outcrops. This is a stock approaching a quartz monzonite in composition, and shows phenocrysts of orthoclase. Disseminated in it are minute grains of pyrite, and cutting it in fine ramifying veinlets is hematite. The alteration of the volcanics along the trail between 2,000 and 3,000 feet elevation and the presence of dikes about the tunnel at 3,300 feet suggest the proximity of an underlying body of igneous rock. The source of the solutions is sought in this more or less hypothetical body of intrusive rock, which is considered to be an extension of the quartz monzonite stock just mentioned.

No detailed examination was carried out to determine the exact character of the alteration of the country rock. It is, however, evident from the nature of the containing volcanics and the present alteration of the wall rock immediately adjacent to the vein proper that calcium, sodium, magnesium and alumina have been substracted locally and in their place silica and the sulphide minerals deposited. These non-metallics may have migrated laterally and formed new minerals, such as occur in propylitization, but no evidence of this is available. The original solutions need not have been high in their silica and iron content because considerable amounts of these would be derived from the replaced rocks, and the silica so obtained would probably be in a colloidal state

as suggested by the experiments of Kahlenberg and Lincoln, on the hydrolysis of the alkaline silicates. The solutions carried silica, iron, arsenic, copper and sulphur along with minor amounts of gold, silver, and barium. During their passage through the volcanic rocks they were likely enriched in magnesium, iron, silica, and possibly barium and CO_2, and later deposited their lode in the veins.

SUMMARY.

This paper is written as a result of a study of the mineral association at the George Gold-Copper Mine, Stewart, B. C.

A brief résumé of the general geology and mineralization of the Portland Canal District, B. C., is given and then a short description of the geology and mineral occurrence at the George Gold-Copper Mine.

This deposit is of interest because of the intricate banded character of the veins, in places, produced through replacement of crushed country rock along and between closely spaced joints in fissure zones; the deposition of quartz in a primary colloidal state with subsequent crystallization and crystal growth to coarse aggregates; evidence supports the view that pyrite was also deposited to some extent in the colloidal state at relatively high temperatures; as well as the occurrence of specularite, altering to magnetite, during an intermediate stage in the sulphide sequence.

The paragenesis determined is pyrite, arsenopyrite, quartz, hematite, and magnetite, and chalcopyrite; this mineral sequence being deposited under intermediate temperatures at a depth approximating 8,000 feet. The last stage of hypogene mineralization was marked by the formation of minute quantities of bournonite (?) in the chalcopyrite and by small amounts of barite.

Due to the simple and constant mineral association it is considered that the deposit was formed under conditions existing in the upper part of the hypothermal zone and at a depth approaching 8,000 feet. The mineralization was confined to one main period, in which there were different stages marked by characteristic changes or pulsations in the mineral deposition, but these

stages were closely related to one another and successive stages overlapped.

The evidence points to the quartz being largely of colloidal origin. Whether silica may be in true solution by itself or in association with colloidal silica remains open for discussion. That pyrite may exist in the colloidal state at relatively high temperatures is likewise indicated.

The formation of specularite and magnetite in this deposit appears to be governed by the chemical environment and to a lesser degree by temperature. The contention that CO_2, released locally from carbonates, has acted as an oxidizing agent, within its limited sphere, is advanced and examples from other deposits are cited in favor of this view. The change from specularite to magnetite is not necessarily due to a rise of temperature but may be caused by local oxidizing conditions reverting to normal reducing surroundings which exist during sulphide deposition.

The writer wishes to express his sincere thanks to Professor Lindgren for his criticism and discussion during the preparation of the material for this paper. He likewise appreciates the suggestions and corrections offered by Dr. H. C. Boydell.

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EDITORIAL

CHINA AND ITS MINERAL RESOURCES.

In an age that has witnessed almost miraculous industrial changes and developments, with a public attuned to romantic achievement, and athirst for sensational novelty, it is not surprising that the roseate hopes of the enthusiastic and articulate young graduates from the Orient, who speak our language and use terms dear to our democratic ears, should be accepted as on the point of realization, viz.: the Eastern giant has awakened; China with a quarter of the world's population is about to duplicate the performance of Japan—already she is engaged in a successful revolution comparable to that of the American colonies, and so the great Chinese democracy, by means of her vast resources of raw materials and man power, will stage an industrial development on American lines which will result in the fiercest industrial war the world has seen, destined to end before the close of the century in the collapse of Western supremacy.

The public is too busy, or perhaps finds the points too abstruse, to realize that there is not a single point of resemblance between the Chinese and the American revolutions; to recognize that China's troubles are internal not external, or to sense the fundamental differences between Japan and China. The Japanese are not supermen, as they would be had they in a generation or two transformed a weak oriental country into one of the few first-class modern powers, but a race, who, with pretty much the same mental and moral qualities, and with pretty much the same essential background as, say, the Anglo-Saxons, starting not far behind the latter and with the Anglo-Saxon pioneer work to guide them, succeeded in little less than the same length of time in duplicating to a striking degree Anglo-Saxon achievement. Strictly oriental countries, on the other hand, have first of all

to overcome a conservatism inbred for millenia, and to develop these same mental and moral qualities before any important, outstanding, progress can be made; also they have to educate half the world's population. In the meantime our familiar and beloved phrases have a connotation that is not grasped by the Oriental, however fluent in our language, and these phrases are meaningless and inapplicable in the Orient.

But such facts, though recognized and subscribed to by practically all competent diagnosticians, are by their nature incapable of expression in exact, concrete figures and are therefore exposed to dispute or contradiction by the uninformed, the prejudiced, or the uncritical, if indeed they are noticed at all. On the other hand the "vast resources in raw materials" upon which industrialization must also be dependent, are tangible and ponderable realities, within the understanding of the public, so that it is a real service to all concerned (and that is the whole world) and to none more than to our oriental brothers themselves, to have the facts regarding these accurately set forth.

This has been done by Dr. C. K. Leith[1] and more recently and more fully by H. Foster Bain.[2] Both show that enough is known of the mineral resources of the East to justify an estimate of their order of magnitude and both point out that the East not only is not rich in minerals but also is deficient in those essential minerals upon which Western civilization is based. America and Europe are the continents with rich resources. This challenges century-old popular belief. Worse than that, it belittles Western accomplishment, for it suggests that it is the country that made the Westerner, not the Westerner that made the country. At most, all the Occidental can claim is that Providence gave the resources to the people who could utilize them. Views so upsetting are apt to be received with incredulity, and it will no doubt be pointed out that 'Feng Sui' still guards the hills of China, whose impious molestation would be followed by dire disaster; that government has paralysed almost all large scale

[1] *Foreign Affairs*, April, 1926.

[2] "Ores and Industry in the Far East," Council of Foreign Relations, 25 West 43d St., New York, 1927.

development, not only by the foreigner with capital and expert knowledge that are so necessary, but also by the Chinese—for instance the Chinese millionaire tin miners of Malay will not leave their mining under the British flag to mine the promising tin deposits under the flag of the Chinese republic; that China lacks the transportation necessary for large scale operations; that the local market is still ridiculously small, the per capita consumption of iron, for example being about one thirteen-hundredth that of the United States; therefore China's resources must be undeveloped, probably largely undiscovered.

Well, geologically, Japan is part of Asia and its mineral resources are fully developed, yet the annual production of this country of 60 million is just about the same as that of Canada, a country of 9 million, that is just beginning to develop its mineral resources. The one Eastern country that is developed is poor in minerals, and it is not, like the Philippines, mantled by recent volcanic rocks. Further, the Chinese have been interested in stones and minerals for many centuries and have good eyes for them. Minor minerals are produced, some of them, like tungsten, only recently valuable and sought for. If these are known and worked, why should minerals known for ages to be valuable remain undiscovered? Moreover, China is not largely unexplored for minerals by Europeans. As Leith and Bain both point out, for several decades Western engineers for the Chinese government or strong Western companies have examined most parts of the country, with results that are meager. Contrast this with the developments, let us say, in Canada, over the same period. To the explorations referred to, the writer might add that Japanese geologists have done a great deal of prospecting in China with similar results and that the late Dowager Empress of China had in her employ for twenty-five years a former member of the Canadian Geological Survey and for three years a second one. These gentlemen had all the information, resources and facilities of the Imperial Court at their disposal. They did not consider China one of the world's great storehouses of mineral wealth. Due to conversations with them on their return, and

to a previous visit to China, the writer was far from holding the popular idea when he went to China last year; yet even so, he was distinctly disappointed in what he saw and learned. True, he covered but a small part of China, and studied in detail only a small corner in the southeast, but he discussed his impressions with the best informed Chinese and Japanese experts and they confirm them.

There is a marked resemblance between the geology of southeast China and western North America. The Japanese recognize Archean, Palæozoic and Tertiary beds in the interior of Kwantung, but have not determined their relationship to the igneous rocks, except that the latter are post-Palæozoic. In Hongkong and the New Territories, Liassic beds are overlain by a thick series of volcanics cut by sills and dikes. These beds were mountain-built and invaded by batholiths of granodiorite presumably about the close of the Jurassic. This intrusion was responsible for some contact metamorphic deposits and possibly for some silver-lead. Considerably later came the intrusion of the Hongkong granite, also following mountain building, judging from the deformation of the granodiorite. This intrusion was responsible for the quartz veins carrying tungsten, molybdenite and some tin. This granite underlies a great deal of the country but is exposed only where erosion has been heavy. In age it may be Laramide or later. Overlying these and apparently laid down on a topography not unlike the present is a late volcanic series cut by dikes and bosses of porphyritic granite that probably represent the vents from which these volcanics reached the surface. This latest granite appears to be responsible for some small silver-lead deposits. While the geology is suggestive of western America, the mineralization in comparison seems slight. So far as the writer can judge, China is deficient in the essential minerals, except coal. She is also without timber, another essential raw material.

But it may be suggested that man-power may offset the material handicap. May it not prove feasible to bring the raw material to the labor? It is not sufficient, perhaps, to state that it never

has. If the labor were sufficiently cheap or exceptionally efficient it might be, but neither of these conditions seem at all probable. The Japanese may not be cited in rebuttal, for, as already stated, they are a northern insular people who from their history and environment have developed qualities much more nearly akin to the British than to Orientals, yet even they have not produced workmen of the efficiency of occidental. The oriental workman has much less efficiency than the Japanese, and climate alone will probably prevent his ever attaining it. Other qualities at present lacking might in time be acquired, but it may be doubted if in the subtropical and tropical climates of the greater part of China, India and the South Sea peninsulas and islands, energy and its offspring, initiative, can be developed as they are in temperate lands.

There remains but one factor, cheapness of labor. It is that and that alone that makes possible the present small beginnings of industrialization. But under industrialization labor speedily organizes, forces up wages and this advantage gradually disappears.

The oriental countries may and probably will develop in time great industries based on their agricultural products, and they will have useful local mineral industries, but it does not seem possible for them to develop into important factors in world commerce along the main lines upon which occidental industry has been built. In other words, if the Orient developes under wise statesmanship, its relationship to the Occident will be one of cooperation in peaceful trade of mutual benefit. Destructive industrial warfare seems an impossibility.

If the above appreciation be correct it follows that the Orient will continue to import the essential minerals and their products, particularly the highly specialized products, and the products of the many industries based upon these, and will continue to export minor minerals in the raw state.

R. W. Brock.

DISCUSSION AND INFORMAL COMMUNICATIONS

METASOMATISM AND THE PRESSURE OF GROWING CRYSTALS.

Sir: Pressure of other work has delayed my reply to Professor S. Taber's long discussion under the above heading, in your issue of November, 1926 (21, pp. 717–727), of my paper "A Discussion of Metasomatism and the Linear Force of Growing Crystals."[1]

My critic's choice of title involving as it does the indefinite phrase ". . . pressure of growing crystals" raises the question as to whether he really grasps the viewpoint of my paper, which envisaged a specific, definitely directed force, *i.e.*, the *linear* force of growing crystals. It seems to me that Professor Taber is confusing this with *volume* force, which was not considered in my paper. Besides the title there are numerous references in his discussion that indicate that he has not discriminated between these two factors. For example, on p. 719 Mr. Taber writes: "All crystals have developed under pressure; minerals crystallizing at great depth below the surface have necessarily grown under enormous pressure." Again on the same page, "Pressure due to increase in volume accompanying alteration of anhydrite to gypsum . . . ," and on page 723, "Since enormous pressure would be required to make room by mechanical displacement . . . the space necessary for crystal growth is usually obtained either wholly or in part through solution of the surrounding minerals. I know of no hypothesis that will explain well-developed crystal form in metasomatic minerals except that the crystals in growing have exerted pressure on the surrounding material, thus causing

[1] H. C. Boydell, ECON. GEOL., vol. 21, pp. 1–55, 1926.

its removal in solution." At the bottom of the same page, "The alteration of magnetite to hematite and of anhydrite to gypsum clearly involves an increase in the volume of mineral matter, and, if the alteration takes place within a rock mass, pressure must be developed." On p. 725, "Water vapor penetrates the pores of the cell and the increase in volume due to hydration results in pressure which ruptures the cell." A few lines further on "When the new mineral is greater in volume than the mineral replaced, the surrounding minerals may be mechanically displaced, or, more commonly, the pressure due to expansion in volume will force into solution either a part of the new mineral or a part of the surrounding minerals and thus remove enough material to compensate for the increase in volume." The above quotations, as well as others, have obvious reference to *volume* pressure. Not once does Professor Taber mention *linear* force or pressure, the factor involved in the pertinent phenomena dealt with in my paper and the one essentially involved if the so-called "force of growing crystals" be a sufficient cause of vein formation.

This confusion is so evident all through the criticism that the only conclusion to be drawn is that Professor Taber has failed to grasp my meaning and hence that he and I are discussing different things. Consequently, further comment on my part will be confined to some other points raised by Professor Taber.

On p. 717 he writes: "The fact that under suitable conditions growing crystals may exert great pressure has been repeatedly demonstrated by experiment and references to 'impossible perpetual motion' do not alter facts." Now the point that growing crystals can exert *volume* pressure was not questioned by me and therefore is not at issue. I do question, however, whether the *linear* pressures that have actually been demonstrated can be correctly considered as "great" geologically, and I suggest that it is in this that Professor Taber and his followers have been led astray through exaggerating the scale of the actual experimental results. With regard to the reference by myself to impossible perpetual motion that Professor Taber takes exception to, I can only say that this is based on the second law of Thermodynamics

which in its turn is usually considered the foundation stone of exact physical and chemical science.

I think, however, that he is quite incorrect when he states, in connection with Le Chatelier's theorem, that ". . . this applies only to a closed system in which concentration by circulation or diffusion from without is impossible and in which pressure is the only variable." The only restriction imposed in the application of this theorem is that the system shall be in equilibrium, a limitation very generally overlooked.

On p. 719, Professor Taber writes: "Growth of crystals under pressure is easier to explain than the method by which replacing solutions penetrate rocks and yet we have abundant evidence that solutions have penetrated the densest rocks." I would comment that capillarity affords a well-based scientific cause for the first penetration of rocks by solutions and that, as Liesegang [2] has indicated and I [3] have emphasized, passage of solution through the metasome affords a reasonable basis for subsequently continued replacement, while the comparatively unlimited linear growth of crystals considered in my paper is as yet quite without adequate explanation.

He misrepresents my meaning when he quotes me as writing, ". . . these distinct limitations . . . would seem to have been quite lost sight of by advocates of the crystallizing force as a factor in vein formation" and then uses this as referring to a particular sentence of mine which he gives. In the preceding quotation by Professor Taber from my paper he omits the words, ". . . imposed in the sentences in italics" and does not mention the sentence to which I particularly referred. This was a quotation from Becker and Day which runs ". . . If the load upon the crystal is too great, or the rate of evaporation (the experiments were carried out in open glass vessels, H. C. B.) slow, the saturation pressure may not be reached anywhere in the supporting liquid layer and growth here may be stopped. A still further increase in the load may even cause re-solution of the bottom sur-

[2] R. E. Liesegang, "Geologische Diffusionen," 1913, p. 15.

[3] H. C. Boydell, "Rôle of Colloidal Solutions in the Formation of Mineral Deposits," p. 229.

face, while the side surfaces continue to grow." This condition does impose a distinct limitation on the efficacy of the linear force of growing crystals as an operative factor in vein formation; if, in holding this view I am in agreement with Becker and Day in so far as limitation of the linear crystallizing force is concerned, how then can I also make "essentially the same error as Bruhns and Mecklenburg" whom Becker and Day showed to be wrong? If Professor Taber had quoted in full what I wrote, his inconsistency would have been manifest from his statement.

On p. 724, writing of gaseous solutions, he states, ". . . but Boydell eliminates gas as a factor in metasomatism without citing any evidence other than the *tendency* of pressure to suppress a gas phase." This is a perversion of my views, which fully appreciate the great influence that gases may have in metasomatism. What I actually wrote was (p. 4): ". . . owing to the existent pressure, the system will have a strong tendency to be a condensed one, *i.e.*, one in which a gas phase is not present." This is undoubtedly true since the influence of pressure is always to drive a chemical reaction, if possible, in the direction of reduction of volume, which, when a gas phase is present, is secured by the reaction proceeding in such a way as to eliminate the gas.

In his conclusion Professor Taber writes: "Those who oppose the theory that pressure due to crystal growth is of importance in geologic processes should show that some other theory will better explain the facts." My paper was directed to showing the inadequacy of the so-called *linear* "force of growing crystals" in vein formation[4] to produce the effects claimed for it by Professor

[4] Since my paper was published, C. W. Correns ("Über die Erklärung der sogenannten Kristallisationskraft," *Sitzungsber. d. Preuss. Akad. d. Wissensch.*, Mar. 25, 1926) has dealt with the "so-called crystallization force." Without distinguishing between linear and volume force, dealing with the dimensions of that force or its influence in vein formation, he thus summarizes his paper:

"Zusammenfassend lässt sich über den sogenannten Wachstumsdruck der Kristalle sagen, dass die Erscheinungen in der Natur und in Experimenten, die bisher einen solchen zugeschreiben wurden, sich entweder durch Volumermehrung bei Änderung des Aggregatzustandes oder durch chemische Reaktionen oder durch Grenzflächenkräfte erklaren lassen.

"Diese Grenzflächenkräfte werden genauer definiert. Die Erscheinung, dass ein Kristall ein aufgelegtes Gewicht hebt, kann zum Verschwinden gebracht werden, dadurch, dass man die Grenzflächenspannungen entsprechend ändert."

Taber. I am still emphatically of the considered opinion, stated on p. 24 of my paper, that the evidence adduced by me effectively disposes of the "force of growing crystals" as an important factor in vein formation.

H. C. BOYDELL.

310 MCKINNON BLDG.,
TORONTO, CANADA.

MAGNESITE DEPOSITS OF MANCHURIA.

Sir: Although Dr. Niinomy [1] has replied to my criticisms [2] of his article on the origin of Manchurian magnesite,[3] it seems to me that he still fails to present convincing evidence of his theory of origin as opposed to mine, and for this reason I venture to make some further remarks on the subject.

Whether the magnesite is a product of replacement of calcareous rocks by magnesium emanating from an intrusive, or of a chemical precipitation (and later recrystallization), is of both scientific and commercial importance, particularly the latter, because of the bearing it may have on the search for new deposits.

I am still opposed to the theory of deposition by magmatic waters because it is in my opinion inconceivable that an acidic granite magma could supply enough magnesium to form the great deposits of magnesite thus far discovered in Manchuria.

If Dr. Niinomy finds it difficult to understand the source of such a large quantity of sedimentary magnesite, how would he explain the widespread occurrence of magnesium carbonate in sedimentary rocks the world over?

I do not believe either that the occurrence of the magnesite at several different horizons in the dolomite is against a sedimentary theory of its origin.

[1] ECON. GEOL., vol. 22, no. 2, p. 195, Mar.–Apr., 1927.
[2] ECON. GEOL., vol. 21, no. 2, p. 190, Mar.–Apr., 1926.
[3] ECON. GEOL., vol. 20, no. 1, p. 25, Jan.–Feb., 1925.

Another fact which I believe supports the sedimentary theory is the occurrence of similar magnesite deposits at several places in Manchuria and China, where there is no visible direct relation between the magnesite and an intrusive rock.

While the writer confesses that he does not know the exact nature of the chemical processes involved in the deposition of the Manchurian magnesite, it seems to him that the following facts observed in the field are of significance. (1) The magnesite is found in metamorphosed sediments such as magnesian limestone, limestone, quartzite, slate and schist; (2) the rocks associated with the magnesite are mostly magnesium-bearing; (3) there is no indication of metamorphism by intrusives other than a few quartz veinlets in the schist lying next the granite; (4) the magnesite occurs at several horizons, with beds of less magnesian character between, and far separated from an intrusive; (5) I see no evidence of primary replacement in any of the magnesium-bearing rocks.

My understanding of the sequence of events is: (1) Erosion and transportation of magnesium-bearing rocks into the then existing Manchurian sea. (2) Local concentration of magnesium carbonates in the sediments. (3) Deposition of a series of sediments of varying composition. (4) Uplifting and metamorphism of the sediments either due to diastrophic movements or granitic intrusions, accompanied in either case by metamorphism of the sediments. (5) During recrystallization of magnesite, the impure portions might be segregated, or the later alteration of the magnesite by percolating carbonated meteoric water containing silica might deposit a film of the latter around the magnesite grains.

Finally because other magnesite deposits of the world showing lithologic and mineralogic relations similar to those found in Manchuria, have been considered as due to the action of magmatic waters, is no reason, in my opinion, why the deposits under discussion should be regarded as having originated in the same way.

I also differ with Dr. Niinomy regarding the nature of the stylo-

litic structure that he refers to as "black lines,"[4] which I understand are thought by him to be of graphitic character. An analysis which I have had made of one of them indicates it to be an impure talc. They are not in my opinion metamorphosed layers of carbonaceous matter, as he claims.

DAIREN, MANCHURIA.

HIRONAO NISHIHARA.

[4] ECON. GEOL., vol. 22, no. 2, Mar.–Apr., 1927.

REVIEWS

Mineral Deposits. By WALDEMAR LINDGREN. 3d edition. 1049 pp., 317 figs. McGraw-Hill Book Co., New York, 1928. Price $7.00.

This classic text and reference book by the dean of American economic geologists will continue to hold its place as the premier work on mineral deposits, for it has just emerged from the press as a carefully revised third edition.

As one glances over the makeup, he notes that as compared with the second edition it is some 92 pages larger, and contains 2 more chapters and 33 more figures. It is also a more pleasing and more sturdily bound volume than its predecessor of nine years ago, and the paper is better surfaced, so that the illustrations reproduce well. But the innovation of a separate "locality" and "subject" index in place of the previous single one is, to the reviewer, a drawback. Invariably he finds himself searching the alphabetical list for the desired index word only to discover when it is not present that he is looking at the wrong index!

But when one delves into the content of the book he sees the results of the thoroughness of the author. Little new material relating to mineral deposits seems to have escaped his reading. Even though the text contains only a few words regarding the new matter, a footnote directs the reader's attention to it, and incidentally the completeness of the footnotes is an important part of the value of this book as a reference. To mention all the additions would be simply to give a bibliographic list of the march of progress of the science of economic geology in the last ten years. But certain noteworthy additions should be referred to. In a new chapter (X.) entitled "The Magma in its Relation to Mineral Deposits," the rather incomplete material of the former Chapter XXIX relating to differentiation, crystallization, and constitution of magmas, is completely rewritten and brought up to date, and there has been added discussion on the "Causes of Vulcanism," "Liquid Immiscibility," "Differentiation by Gas Transfer," the "Relation of Volcanoes and Lava Flows to Mineral Deposits," "Relation of Intrusive Magmas to Mineral Deposits," "A General Review of Ore Deposits with Magmatic Affiliations," and the "Zonal Theory." This chapter now removes a pronounced defect in the last edition; it assembles concisely the new material on this subject, though the subject of differentiation is perhaps too brief. In the opinion of the reviewer, it is the most outstanding contribution to

this third edition, though he would prefer to see even more space devoted to the theoretical consideration of this important phase of economic geology. The other new chapter (III.) is the "Atmosphere, Hydrosphere, and Underground Water." It seems hardly necessary.

The subject of gold quartz deposits has several pages added to it describing the Northern Ontario gold deposits. Likewise the new information relating to platinum deposits (pp. 873–876) in consequence of the South African discoveries, is incorporated. Also there is considerable addition as well as revision of material relating to lead-zinc deposits in sedimentary rocks. Under the chapter on "Texture of Mineral Deposits" the subject of diffusion and unmixing from solid solutions is introduced. There are some geologists, the reviewer among them, who will not agree that diffusion can be relegated only to short distance work. Throughout the whole book, one notices by the addition of a word here, a sentence there, or a paragraph or page, Professor Lindgren's tendency in recent years to give prominence to the rôle of colloids and gel replacement in the formation of mineral deposits. The treatment of oxidation of metallic ores has been carefully gone over, but the reviewer cannot agree with the statement (p. 949) that "covellite has not yet been proved a hypogene mineral in sulphide deposits of hypogene origin." Kennecott, Butte, and Magma, offer too strong evidence to the contrary.

In the chapter on "Concentration in Molten Magmas" Professor Lindgren has swung away from the idea of magmatic segregation *in situ* of magnetite ore; he says:—"It looks, therefore, as if the liquid magmatic iron ores were very scarce." and he thinks the Sudbury deposits for the most were injected masses of sulphide melts, the products of an earlier differentiation. In a chapter that has undergone so much revision, it is rather surprising to find no reference to Vogt's[1] recent work on the Kiiruna iron deposits of Sweden. Nor, under Corundum, is there any mention of the important South African occurrences; or of the new ideas relating to the origin of corundum in magnesian rocks by desilication.[2] Also in the discussion of the gold of the Rand Banket, the recent important paper of Reinecke[3] has been overlooked.

[1] Vogt, T. H., "The Kiiruna Type," *Geol. For. Forhand*, Stockholm, B. 49, Ht$_2$. 1927.

[2] Du Toit, A. L., "Plumasite (Corundum Rock) and Titaniferous Magnetite from Natal," *Trans. Geol. Soc. So. Africa*, vol. 21, 1918, pp. 53–73.

Hall, A. L., "Corundum in Northern and Eastern Transvaal," Union of S. Africa Geol. Surv. Mem. 15, 1920.

Gordon, S. G. Desilicated granite pegmatites, *Proc. Nat. Acad. Nat. Sci.*, Philadelphia, part 1, 169–192, 1921.

Cobb, Margaret C., Corundum with Dermite in N. Carolina. Doctor's Dis-

This edition contains no such pronounced changes as occurred in the second edition. The order of the chapters has been changed a little (and with benefit), and material has been changed from one chapter to another, as for example, the description of the Butte ore deposits has properly been placed under the deposits formed at intermediate temperatures instead of under oxidized deposits. But one's attention is caught at once by the new terminology; shallow deposits are now epithermal; intermediate are mesothermal; high temperature are hypothermal; and contact metamorphic deposits are pyrometasomatic. These terms have the advantage of brevity, but are almost as indigestible as W. H. Emmons' new terms relating to zoning. Our science cries for relief from new and strange terminology; unless the fashion changes, geologists in another decade will almost blush to speak an Anglo-Saxon word.

As one leafs the pages and notes the careful revision of material and inclusion of new occurrences, he wishes as careful attention had been given to the unrevised parts, and a phrase changed here and there, for when he reads: "a recent description," and the footnote date is 1910, the confidence of some in the recency of the material might be shaken. Or when it says: "an important deposit of secondary chalcocite is now being worked on a large scale at Ely, Nevada," (p. 953), the reader might not realize that already for twenty years this has been one of the great mines of the country. And is Morro Vehlo (p. 187) really the deepest mine, or is not the Village Deep?

But such features are few and trivial compared to the excellence of the book. The subject matter of the previous edition is too well and favorably known to need comment here. Now that it is revised again, it will continue to hold the premier place as the most accurate and reliable reference book on mineral deposits for the student and engineer. In my opinion it is the outstanding book on mineral deposits in the world. We all owe thanks to Professor Lindgren for his labors.

The publishers deserve credit, too, for the book is a fine bit of craftsmanship, but when I note the price of $7.00, I wonder if book-making has not fallen behind other industries in manufacturing efficiency and lowering of costs.

ALAN M. BATEMAN.

sertation, Bryn Mawr College, privately published, 1924.

Hall, A. L., and Nel, L. T., "On an Occurrence of corundum-Sillimanite Rock in the Norite of the Bushveld Igneous Complex," *Tr. Geo. Soc. So. Afr.*, vol. 29, 1926, pp. 1–16.

[3] Reinecke, Leopold, *Trans. Geol. Soc. S. Africa*, vol. 30, 1927, pp. 89–119.

Alluvial Prospecting, The Technical Investigation of Economic Alluvial Minerals. By C. RAEBURN and HENRY B. MILNER. xix + 478 pages, 139 figures and 32 plates. Thos. Murby & Co., London, 1927. Price 36 s.

With the increasing difficulty of discovering valuable materials in alluvial deposits it seems necessary to the authors of this book to systematize the procedure in searching for such materials, and to furnish for the prospector such aids as will enable him to better attain the information he is continually seeking. Their aims are "to create the atmosphere of interdependency of prospector and petrographer; to advance the case for systematic exploration, valuation and determination of economic alluvial minerals; to stress the limitations of field-analysis and the safer routine of the laboratory where requisite; to interpret and reanimate those first principles of physical geology which lead to rapid and accurate selection of probable mineral concentrations, and, incidentally, to a lasting grip of the essentials of this phase of applied geology."

"Readers familiar with recent work in sedimentary petrology will at once appreciate the potentialities of adapting the now well-established principles of that science to the economic requirements of alluvial work; at the same time they will realize that the practice of such technique demands at least a working knowledge of thorough mineralogical analysis which, as a matter of fact, today forms an essential part of the mining geologist's training."

Consequently the authors offer a philosophical discussion of the origin of alluvial deposits, and excellent description of the characteristic features of those produced by different processes, and a very comprehensive account of the assemblages of minerals that may be encountered in them.

More than half of the volume is devoted to the field examination of alluvial concentrates and their laboratory study. The method of securing concentrates is explained in detail and lists are given in which are classified the characteristic alluvial minerals based on luster and color.

Perhaps the most valuable portion of the volume is that dealing with the methods of determining the nature of the minerals in the concentrates. The technique employed in separating concentrates into their component parts is discussed in great detail. The use of sieves and magnets for this purpose is described, and electrostatic and dielectric methods of separation are briefly mentioned. Then follow references to vibration methods and a rather full discussion of the use of heavy liquids. Many novel methods for determining the physical characteristics of minerals in grains are given and rather full accounts of the technique employed in determining their chemical properties and their indices of refraction.

One hundred and twenty-five pages deal with the diagnostic properties of seventy minerals.

The book ends with a glossary and a bibliography of about two hundred entries.

The volume is a complete manual for the use of those interested in the investigation of sediments, whether unconsolidated as in alluvial deposits, or consolidated as in sandstones and shales. It is well printed and well illustrated, and is abundantly supplied with references to authoritative articles on all the subjects discussed. It should be of great value to the sedimentationist, the stratigrapher, the petrographer and the oil geologist.

W. S. B.

The Eötvös Torsion Balance. Anonymous (probably CAPT. H. SHAW and E. LANCASTER JONES). Published privately by L. Oertling, Ltd., as a manual to be used with their torsion balance. 90 pages, London, undated, but late in 1925 or early in 1926.

The content of the book covers: I. The theory of the Eötvös torsion balance; derivation of the working formulæ; description of the instrument; method of observation; relation of the magnitudes observed to the lines of force and the level surfaces of gravity; corrections for normal effects and for near and distant terrane; subterranean effects; effects due to certain mathematical bodies. II. Notes on the practical employment of the Eötvös torsion balance; initial testing of the instrument and its accessories; hints on planning the survey; work at the station site; office computation and plotting of results; the interpretation of the results. III. A description of the Oertling instrument and instructions for its manipulation.

This book was the best discussion in English on the theory and use of the Eötvös torsion balance, and is surpassed only by a later paper by the same authors that covers much of the same ground. The book is suitable for a textbook for beginners, and advanced students will find it of use. It should be available as a reference book in all geological libraries and in all torsion balance department offices.

The discussion is authoritative and is given as simply as it is possible to give theory that is not simple. Formulæ for the terrane correction calculated by Schweydar's method are given in terms of tenths of feet difference in elevation and may be welcome to some who find it inconvenient to survey in the station positions in feet and then take the station levels in centimeters.

DONALD C. BARTON.

HOUSTON, TEXAS.

Chemische Geologie. By FRITZ BEHREND and GEORG BERG. 595 pp., 61 figs. Ferdinand Enke, Stuttgart, 1927. Price bound, 40.40 marks.

This volume is somewhat broader in scope than the title indicates; besides the chemical aspects many of the physical features of the subject are treated. The book is divided into six parts. Dr. Berg has written sections on the chemistry of the earth as a whole, the chemistry of magmas, the chemistry of metamorphism, and the chemistry of magmatic exhalations. The last section includes much material that is of interest to the economic geologist. Dr. Behrend is responsible for sections on weathering and on formation of sedimentary rocks. This book is an exceedingly valuable contribution to the subject. It has a place in the field that is partly occupied by Boeke-Eitel, and "The Data of Geochemistry." Incidentally it might be noted that the citations from "The Data of Geochemistry" are from the 1916 edition. Source references are numerous, recent, and well chosen, making the book more valuable.

F. F. OSBORNE.

Ores and Industry in the Far East. By H. FOSTER BAIN, with a chapter on Petroleum by W. B. HEROY, and a preface by EDWIN F. GAY. Published by the Council on Foreign Relations, Inc. New York, 1927. 216 pages, 19 figures.

This book strikes at the heart of the popular fancy that the far eastern countries contain a vast reserve of metals. It presents a survey of the mineral resources of China, Japan, Eastern Siberia, Indo-China, Siam, the Netherlands East Indies, The Federated Malay States, and the Philippines and comes to the rather startling conclusion that, excepting coal, tungsten, antimony and tin, there are no ores of the metals in those regions that give promise of developing into important contributors to the world's supply.

The truth of this fact has been known to those most interested for some years. The many sources of information are given in this book in the form of a bibliography by J. W. Frey and the chief value of the book arises from the fact that these scattered references are moulded into compact and readable form and the reserves of all the important minerals in the many countries listed above are discussed and summarized under one cover. Throughout the book, the author's own knowledge of, and experience in, the Far East is clearly evident.

The following paragraph from F. R. Tegengren quoted on page 96 gives an idea of the relative scarcity of the all important metal iron in the vast realm of China: "The total quantity of iron ore (both actual and potential) represented by the figures above would be consumed by the iron industry of the United States within less than nine years." The other states considered are similarly poor in iron.

The condition in relation to petroleum is somewhat better but the Netherlands East Indies is the only part of the area that now furnishes an appreciable part of the world's supply and that is believed to be capable of considerable expansion.

The last chapter discusses the probable future development of these countries and states that while no attempt is made to discuss the question of the necessity or the desirability of the East following in the footsteps of the industrialized West, changes along those lines are already apparent and the question arises as to how the needs of such a changing civilization are to be met. Italy is given as an example of a western state that has played a major part in the world's history with far smaller mineral wealth than Japan—an eastern state approximating it in size and population. Due allowance is made for sources of national wealth besides minerals, still the conclusion seems inevitable that, if and when the East becomes industrialized, in place of large additions to the mineral wealth of the world, an added burden will fall upon the resources of the West.

YALE UNIVERSITY, W. M. AGAR.
NEW HAVEN, CONN.

Map of the Mineral Deposits of the World. Edited by the Prussian Geological Survey; Scale 1 : 15,000,000; 8 sheets. Supplementary book of tables; 312 pp. Dietrich Reimer-Ernest Vohsen, Berlin, 1927. Price 120 marks. Or International Map Co., New York, price $35.00.

The map consists of eight folded sheets, each 33½ x 41 inches, contained in a suitable folder. Each sheet thus covers one eighth of the world. The known mineral deposits and their containing rocks in each part of the world are shown in their proper location by suitable symbols, colors and letters. For example, a triangle designates a metasomatic deposit; three sizes indicate whether of world importance, intermediate importance, or unimportant; a violet triangle designates younger eruptive rocks; yellow inside the triangle indicates it is a precious metal or quicksilver deposit; and *Au* shows it to be gold. Each sheet contains a legend. The accompanying tables, arranged geographically, list each deposit (designated by a number on the map) by name, and record the host rock, type of deposit, grade of ore, 1913 and 1923 production, and map coordinates. A glance at the map tells the deposits present in a given locality, and reference to the tables enables one quickly to gain data regarding them.

It is a stupendous piece of work, painstakingly done, and of great value to geologists, mineral resource bureaus, and mineral companies who desire restricted information about the metallic and non-metallic deposits of different parts of the world. The Prussian Geological Survey is to be commended for undertaking so great and useful a labor, and the publishers, for an excellent publication. ALAN BATEMAN.

BOOKS RECEIVED

Kohlenpetrographischen Praktikum. By DR. ERICH STACH, of the Prussian Geological Survey. 196 pp., 64 figs. Gebrüder Borntraeger, Berlin, 1928. Price, 10.80 marks.

This is Volume 14 of the Sammlung naturwissenschaftlichen Praktika. A thorough microscopic study of coals by means of transmitted, reflected, and polarized light. Well written, excellently illustrated, and well printed.

Geology of the Gold-Pyrite Belt of Northeastern Piedmont, Virginia. By JOHN T. LONSDALE. 110 pp., 11 figs., and maps. Va. Geol. Survey, Bull. 30, University, Va., 1927.

A good description of the geography, physiography, rock formation and mineral deposits, including gold, pyrite, copper, iron, graphite, soapstone, building stones and road materials.

Optic and Microscopic Characters of Artificial Minerals. By A. N. WINCHELL. 215 pp., 9 charts, determinative tables. Univ. of Wisconsin, Studies in Science No. 4. Madison, 1927.

A supplement to Part II. of the author's "Optical Mineralogy." A thorough treatment of the artificial minerals giving their chemical name and composition, crystal system, physical characteristics, optic properties, inversions and synthesis.

Geography, Geology, and Mineral Resources of Part of Southeastern Idaho. By G. S. ROGERS. With descriptions of Carboniferous and Triassic Fossils by G. H. GIRTY. 453 pp., 70 plates, 46 figures. U. S. Geol. Surv. Prof. Paper 152. Washington, 1927.

A comprehensive treatise of the geology of the Western phosphate reserve in Idaho.

The Properties of Silica—An Introduction to the Properties of Substances in the Solid Non-conducting State. By ROBERT B. SOSMAN, Geophysical Laboratory. 856 pp., 40 figs., index. Chemical Catalogue Co., New York, 1927. Price, $12.50.

A monographic treatment of the properties of silica in its various forms; includes all that is known of the physical properties; authoritative (to be reviewed later).

Allgemeine Petrographie der "Olschiefer" und ihrer Verwandten (mit Ausblicken auf die Erdölentstehung). By DR. ROBERT POTONIE. 173 pp.; 27 figs., index. Gebrüder Borntraeger, Berlin, 1928. Price 12 marks.

A careful petrographic treatment of oil shales and bitumens and enclosing rocks.

SOCIETY OF ECONOMIC GEOLOGISTS

PENROSE MEDAL PRESENTED TO J. H. L. VOGT

On Monday evening, February 20th, the Penrose Medal of the Society of Economic Geologists was awarded to Professor J. H. L. Vogt, of Trondhjem, Norway. The presentation was made at a dinner of the Society held in the Engineers Club, New York, during the meetings of the American Institute of Mining and Metallurgical Engineers. Mr. Hans H. T. Fay, Royal Norwegian Consul General, received the medal for Professor Vogt. The medal was presented for the Society by Professor Waldemar Lindgren who reviewed the progress made in the study of ores closely allied to magmatic processes, and showed in what large part Prof. Vogt contributed. Professor Lindgren's address of presentation will be printed in full in an early number of Economic Geology and will be distributed to members of the Society.

In receiving the medal Mr. Fay called attention, most happily, to the international aspect of scientific work, and to the pleasant personal contacts established thereby. He told of the work of the Trondhjem Technology Institute, and of Professor Vogt's personal influence. Mr. Fay's own presence did much to make up for the absence of Professor Vogt himself.

The following were present at the dinner: H. F. Bain, A. M. Bateman, C. P. Berkey, J. M. Boutwell, A. D. Brokaw, E. L. Bruce, E. F. Burchard, E. DeGolyer, H. H. T. Fay, E. J. Fohs, J. A. Grimes, A. O. Hayes, F. L. Hess, D. F. Hewett, Ernest Howe, Adolph Knopf, Waldemar Lindgren, G. F. Loughlin, G. R. Mansfield, B. L. Miller, H. D. Miser, E. S. Moore, Edward Sampson, J. T. Singewald, W. T. Thom, Jr., A. C. Veatch and David White.

President Emmons has appointed the following Executive Committee: S. H. Ball, A. D. Brokaw, E. E. Ellis, H. G. Ferguson, J. V. Lewis, Edward Sampson.

The program committee for this year consists of G. F. Loughlin, Chairman, E. S. Bastin, B. S. Butler, Adolph Knopf, Augustus Locke.

H. G. Ferguson has been appointed to represent the Society on the Division of Geology and Geography of the National Research Council. He takes the place of M. R. Campbell who retires. The other representative of the Society is F. L. Hess.

Alan M. Bateman has been appointed the Society's representative to the Council of the Geological Society of America.

SCIENTIFIC NOTES AND NEWS

L. C. Graton is leaving South America for South Africa.

George Otis Smith has been instated as President of the American Institute of Mining and Metallurgical Engineers for the year 1928.

Harry von Eckermann has been made a part-time lecturer in geology at Stockholm University, Sweden. He plans to continue his private work. His address is now Edeby, Sparreholm, Sweden.

H. C. Boydell has entered into consulting partnership with Douglas G. H. Wright, under the name of Wright and Boydell, with offices at Rooms 310–311 McKinnon Building, Toronto, Ontario, Canada.

W. E. Wrather has removed from 6044 Bryan Parkway to 4300 Overhill Drive, Dallas, Texas.

Sidney Paige has gone to Caracas, Venezuela, to make geological researches for the Venezuelan Government.

George H. Garrey recently spent two months at the Pierpont Mine near Ely, Nevada, and has now returned to his office in Philadelphia.

Alexander Duessen has resigned as vice-president of the Marland Oil Company of Texas and has re-opened his office as consulting geologist at 1606 Post-Dispatch Building, Houston, Texas.

Victor A. James, for several years consulting engineer with the Exploration Department of Dome Mines, has joined the firm of Wright and Boydell, Toronto, Ontario, as one of the staff.

James Rattray is now mine manager, and Douglas Wright geologist, of Kirkland Eastern Gold Mines Ltd., in northern Ontario.

Frederick G. Clapp has completed a reconnaissance of four northern provinces of Persia, with special attention to the question of oil occurrences.

Fred Searles of the Newmount Mining Company is at present in South America.

Samuel H. Dolbear, consulting mining engineer, is now located at 17 Battery Place, New York City.

A. P. Serecrowsky, of the Mining Academy of the University of Moscow, Russia, has been visiting the mining districts in California.

Leslie Urquhart, chairman of the board of directors of Russo-Asiatic Consolidated, Ltd., sailed from London in January to visit the Mount Isa silver-lead mines, Queensland, which are controlled by his company.

F. Julius Fohs returned to this country in January after six weeks in Europe.

The Schlumberger Company has made an electrical survey of a 1,300-acre tract on Cape Breton Island for the Consolidated Mining and Smelting Company of Canada.

Philip D. Wilson is spending a few months in South Africa.

The American Association of Petroleum Geologists has been invited by New Orleans to meet in that city in 1929.

The German Geological Society will hold, for the first time since the War, a meeting devoted exclusively to economic geology, at Eisenach, Thuringia, on May 18 and 19. The principal subject of discussion will be the potash deposits of Germany.

The United States Geological Survey has lately collected fundamental data on the potential water power resources of the Umpqua River Basin, Oregon, supplemented by a field examination by B. E. Jones, hydraulic engineer, and H. T. Stearns, geologist, who have assembled and analyzed the data in a manuscript report. Some 247,000 H. P. are available for 90 per cent. of the time, and 584,000 H. P. for 50 per cent. of the time, without storage. With unified operation, available power for 90 per cent. of the time would be over 400,000 H. P. This report may be examined at the office of the Survey, Post Office Building, Portland, Oregon.

The Survey has also issued a statement showing that the developed water power of the United States on January 1, 1928, was 12¼ million H. P., an increase of over one half million H. P. during 1927. The total potential power available 90 per cent. of the time is 38 million and for 50 per cent. of the time is 59 million H. P. California ranks first, New York second, and Washington third. Complete data may be obtained from the Director of the U. S. Geological Survey, Washington, D. C.

The American Institute of Mining and Metallurgical Engineers held its 136th meeting in New York, February 20–23. So many papers were on the program that as many as five sessions were running simultaneously. The most popular sessions were those on Geophysics, presided over by E. L. DeGolyer and Max Mason, where the following papers were presented:—The Eötvös Torsion Balance Method of Mapping Geologic Structure, Donald Barton; Gravity Surveying in Great Britain, H. Shaw; The Computation of Eötvös Gravity Effects, E. Lancaster-Jones; Cartographic Correction for the Eötvös Torsion Balance, C. A. Heiland; Experiments with Eötvös Torsion Balance in the Tri-State Zinc and Lead District, P. W. George; Electrical and Magnetic Methods, Max Mason; Geophysical Exploration for Ores, Max Mason; Remarks on Magnetic Method of Survey, L. B. Slichter; Certain Applications of the Surface Potential Method, Warren Weaver; Recent Results in Electrical Prospecting for Ore, Hans Lundberg; Earth Resistivity Measurements in the Lake Superior Copper Country, W. J. Rooney, W. O. Hotchkiss and James Fisher; A New Micro-magnetometer, Frank Rieber; Working

Method of Practical Geophysics, Hans Haalck; Radiore Process, Its Application in Canada, Edward H. Guilford; Some Applications of Potential Methods to Structural Studies, E. G. Leonardon (by title).

At the session on Mining Geology the following papers were presented:—Ore at Deep Levels in the Cripple Creek District, Colorado, G. F. Loughlin (by title); Geology of the British Guiana Bauxite Deposits, L. T. Emory; Geology of the Red Lake and Woman Lake Gold Areas, Northwestern Ontario, E. L. Bruce; Research in Processes of Ore Deposition, Waldemar Lindgren; The State and Density of Solutions Depositing Metalliferous Veins, W. H. Emmons; Sedimentary Metalliferous Deposits of the Red Beds, John W. Finch; Geology of the Moffat Tunnel, T. S. Lovering; Ore Deposits Support Hypothesis of a Central Arizona Batholith, I. A. Ettlinger (abstract); Deposition of Ore in Preexisting Limestone Caves, R. T. Walker (abstract).

Lively discussion took place, particularly following Professor Lindgren's paper.

John A. Burgess has returned from a geological examination in Sinaloa, Mexico.

T. Mackintosh Bell has resigned as managing director and consulting geologist of the Huronian Belt Co. Ltd., Ontario.

A. C. Spencer of the U. S. Geological Survey is at present in New Mexico completing his Survey work there.

V. A. Tames has resigned from the exploration department of the Dane Mines to join the firm of Wright and Boydell, Toronto.

Waldemar Lindgren, at present chairman of the section of Geology and Geography, National Research Council, Washington, is engaged in establishing a journal of Annotated Bibliography of the International Literature of Economic Geology.